Annual Subscription Price, $3. 94 *Published Ten Times a Year.*

The works of the Lord are great, sought out of all them that have pleasure therein.—Ps. cxi, 2.

THE SIDEREAL MESSENGER.

APRIL, 1891.

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The Geometry of Position, by Robert H. Graham. London and New York, Messrs. Macmillan & Co., Publishers.

WM. W. PAYNE, EDITOR,

Director of Carleton College Observatory.

NORTHFIELD, MINN.

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THE SIDEREAL MESSENGER,

CONDUCTED BY WM. W. PAYNE.

DIRECTOR OF CARLETON COLLEGE OBSERVATORY, NORTHFIELD, MINN.

VOL. 10, NO. 4. APRIL, 1891. WHOLE NO. 94

AN IRREPRESSIBLE CONFLICT.

LEWIS BOSS.*

FOR THE MESSENGER.

From the issue of the first volume of the publications of our government Observatory in Washington, the antagonism between "U. S. National "and "U. S. Naval" may be said to have begun. Secretary Bancroft had christened it "National"; but in the Observatory volume for 1845, which bore on its printed title-page the right name, "National," another was interpolated upon which was engraved the word "Naval." In the next few years the term "National" asserted its supremacy, until in 1854 the Secretary of the Navy, Hon. J. C. Dobbin, settled matters in these words: . . . "My opinion is that it should be styled 'The U. S. Naval Observatory and Hydrographical Office.' . . . It is a Navy affair, and its reputation is the property of the Navy. If it assume another name and character, *the next step will be to place a civilian at its head.*" The italics are mine. Secretary Dobbin was doubtless thinking of the memorable efforts of John Quincy Adams to establish a National Observatory, of his efforts to secure the interest of the Smithson fund for that purpose, and of the clear-sighted views of that great statesman, who said: "The express object of an Observatory is the increase of knowledge by *new discovery.*" (The italics are not mine in this case.) He might also have suspected or even have known that what little reputation the infant institution had gained during the first seven or eight years of its existence had been the merit of civilians like Coffin, Hubbard and Walker, whose lot at the Observatory had been far from a happy one—a fact which we are permitted to suspect had not altogether escaped the knowledge of Secretary Dobbin.

*Director of Dudley Observatory, Albany, N. Y.

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How vital to the reputation of the new Observatory had been the services of a few of the leading civilians employed there may be judged from the almost total eclipse of that reputation which occurred during the fifties, when only a small contingent of civilians like Yarnall and Ferguson remained in the atmosphere of the quarter-deck. One reads with amazement the "details" for the instruments in those earlier years,—the only astronomers of the institution serving at the foot of the lists, under the direction of lieutenants of the Navy. Is it any wonder that the "zones" which were observed in those years by direct order of Secretary Bancroft (March 6, 1846) are without a rival in that period, either for inadequacy in amount of observing, inaccuracy of the observations, or good-for-nothingness generally. Yet a line officer of the Navy, a trained "executive officer," was in charge of each instrument. And what an inspiration to work lay in that order of Secretary Bancroft. Referring to the report of Lieut. Maury, your observations to further the interests of navigation are well enough, he said, but "The country expects also that the Observatory will make adequate contributions to astronomical science;" you must take up the observation of small stars where Bessel and Struve left off, and with the advantage of your more southern latitude, carry these to the southernmost practicable limit. It was a good program and the matter was particularly timely. It was a trumpet call calculated to inspire the heart of any astronomer who had feared that our government would not lend its open sanction to the work of original research. Had it been carried out with skill and effective zeal, what a monument to the credit of American astronomy might the results have been, instead of the reproach which they actually are. Unfortunately the president at that time concurred with Bancroft's wish and sent him abroad six months after the issue of the order that directed the National Observatory to work on the zones. Had he remained at his post until 1849, it is quite possible that Walker, or some equally capable, practical astronomer, would have been placed in charge of the Observatory, with results vastly different from those which have been experienced.

It is true that some respectable work was done at the

National Observatory during the first four years of its existence (but not on the zones). That of the next four years is practically worthless, except some of the work with the 9.6 inch equatorial. Afterward, until 1861 the astronomical history of the Washington Observatory is almost a blank except for the observations made by two or three civilian members of the staff, who kept the lamp of science burning under a bushel, as it were, during the whole of that lamentable period.

It is true, also, that Lieutenant Maury, in his capacity as chief of the Hydrographic Office, made those remarkable investigations in regard to the winds and currents of the ocean which will cause his name to be forever remembered among nautical men. But these investigations were no part of the work of an astronomical Observatory, and they form no part of its actual record. They prove how efficient a man can be in directing scientific work for which he has aptitude and special training and experience; and, by contrast, how ignominiously the same man may fail, even with the aid of able assistants, when he attempts to direct scientific work for which he has not been thoroughly educated and trained, and in which he has had no adequate practical experience.

How much better a practical astronomer can direct the labors of an Observatory was well illustrated in the period, 1861 to 1865, when Captain Gillis became superintendent of the Naval Observatory. Perhaps the best proof of his wisdom and efficiency was exhibited in his choice of new assistants, and in the rapid recognition of their special talents. Whatever confidence he may have had in the astronomical effectiveness of the versatile young officers of the Navy was probably destroyed by an inspection of the work which they had done for him, as his assistants in the U. S. Naval Expedition to Santiago de Chile. The good effects of his administration, reinforced by the favorable circumstances of the next two years, under the superintendency of the accomplished Admiral Davis, lasted for years after that time, and this constitutes the golden age of the Washington Observatory. Captain Gillis may not have been entitled to rank among the ablest astronomers which this country has produced, but he was nevertheless an astronomer, and felt an

earnest and intelligent interest in the scientific reputation of our National institution.

It is not strange that about the time of his death, the old desire for a National Observatory should have flickered into life again. We see the evidences of this in the Act of Congress, approved March 3, 1865, by which the "rider" in the appropriation bill of 1848, providing that the superintendent should be a Naval officer, was repealed; and stands repealed up to the present time. With the history of this legislation, and with the plans and efforts by astronomers of that time to bring about a reform in the system of management at the Naval Observatory, we of the younger generation are not generally initiated, and it may not be worth while to inquire. But we do know that from that time to the present there has not been an hour when the astronomers of the country have felt that the National Observatory can be the representative of American astronomy that it ought to be, and that the public have the right to expect it to be, under any other than a responsible and competent scientific head.

Twice within the last dozen years the astronomers of the country have raised protests against the present system of management; and once their wishes came near realization in part, failing only for reasons which none of us care to recall to mind. In neither case was the issue direct, or the trial on merits satisfactory.

It was in 1865 that the rule of a line of distinguished Naval officers of high rank was inaugurated. During this period of nearly twenty years, no very serious complaint on the part of astronomers was heard during the incumbency of a superintendent. Men of great personal dignity of character, of national reputation, and who in long terms of professional service, some of it passed in the great struggle for national integrity, had deserved well of their country,—these men could not be seriously antagonized. *Silent leges inter arma.* It was in the interval between the administrations of Admirals Rogers and Rowan that the first well organized movement to place an astronomer in charge of the Observatory was made. But in 1885, astronomers felt no such personal check as had been implied by the incumbency of these distinguished Admirals, whom a grateful country delighted to honor.

The system had been a bad one for the true interest of astronomy in spite of the liberal and judicious executive management of those distinguished men. The Observatory has always been, since 1865, an astronomical mob, sometimes successful in attaining the most brilliant results by individual prowess; but always lacking in co-ordination of work, and wasteful of effort; excelling in theoretical rather than in practical astronomy; sending forth beautiful researches rather than refined and well digested results of observation; relying for excellence and its rewards upon the efforts of a few men working practically without any direction whatever, rather than upon the disciplined and well directed efforts of an entire staff.

It is true that much of the observing work of our national institution is good, and much of it is excellent and even unsurpassed. For the most part, the subordinates at the Observatory are as earnest, faithful and capable as are the men in similar relations in any other great national institution of the kind. But any one who is acquainted with the inner workings of that establishment knows the peculiar difficulties under which an astronomer must labor. Placidity and longevity are the stepping stones to material success, which no zeal, no eminence of acquirement can hasten, and no laziness nor incompetence delay. It is idle to assert that astronomers are not, like all the rest of mankind, to some extent influenced by the hope of material rewards. There is probably no class of workers more indifferent to the matter of pecuniary compensation than astronomers are; but he would be a rare and peculiar man indeed who would not hold and justly hold, that the material rewards of intellectual achievement must be to some extent proportioned to the quantity and quality of results produced. One would scarcely have the hardihood to deny, that in the human relations which astronomers, in common with all other men, are bound to form, that the hope of pecuniary reward even for the highest and most unselfish intellectual effort, is recognized as an incentive. Yet the young astronomer entering the corps of Professor of Mathematics, U. S. N., comparatively undeveloped and untried, sees his entire material future marked out for him in undeviating lines. The most ordinary good behavior, the most perfunctory attention to

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duty, will secure this future for him quite as certainly as will the genius of a Newton, or the colossal labors of a Herschel. To be sure the main incentives still remain,—the pride of achievement, and that which is the most powerful of all, the love of scientific investigation in and for itself; but these are not alone what governments must rely upon for securing the most uniformly effective and economical service from those whom it employs.

Probably the management of our Government Observatory by almost any competent astronomer would be more exacting in requirements than the management now is. It is not likely that any astronomer at the Naval Observatory, in recent years, could justly complain of overwork exacted from him, or of any lack of considerate courtesy from his superiors of the Naval line. But for the sake of illustration, one may easily imagine that the feelings of an astronomer on the Naval Observatory staff must be very much akin to those which the passengers on an ocean steamer would feel, if an astronomer who had passed all his life upon land were invested with supreme command on the bridge. The proposition needs only to be stated to an astronomer in order that its full force shall be appreciated. During the last decade there has been a distinct retrogradation at the Naval Observtory in the output of results as well as in *morale*. There is probably not a practical astronomer in the land who does not realize this fact. During the last year or two, there have been increasing indications that the old, irrepressible conflict is about to break out again with renewed force. The epoch of proposed removal from the old to the new site is rapidly approaching. Before, or soon after this is effected, arrangements must be made which will be likely to fix the character of the National Observatory for a long time to come. Whether anything will be accomplished to make this institution an object of pride to American astronomers and the country at large rests with astronomers themselves. The responsibility cannot be evaded. Now while astronomers universally recognize the righteousness of this cause, one constantly hears the time-serving caution: "The Navy is too strong." The writer himself does not claim exemption from having entertained this feeling. But it should be remembered that in all history the rightfulness of a cause is

the strongest element for ultimate success. Organization, numbers, interest at court, superior finesse in inducing opponents to adopt arguments and propositions destructive of their own reasoning and in promoting divisions in the ranks of astronomers, may postpone a correct decision on the merits but cannot ultimately prevent it. The conflict is irrepressible.

Congress is a fair-minded tribunal when its attention can be directed to a subject like this. That has been emphatically proved by its action in reference to the antagonism between civilian and army surveys for geographical and geological purposes. The Army contended for its supposed prerogatives quite as sturdily as the Navy can hope to do in the case of the Naval Observatory. The Army could produce a score of weighty arguments in its favor in the former case where the Navy can produce one in the latter. And yet Congress made short work of the matter when its attention was once effectively drawn to the point.

The history of the transfer of the Weather Service to the Agricultural Department, under a civilian organization is another illustration of the manner in which Congress deals with this class of questions when it is made to understand the merits. The agitation in this case had to be kept up for a longer time than was necessary in the matter of the geological and geographical surveys, but the final result was the same, and the new organization, civilian throughout, will soon go into operation. Yet with its large *personnel*, and with the complicated business details of the Weather Service, the argument for military discipline and for the necessity and propriety of making mere executive capacity the chief function of its responsible head was ten times as strong in this case as it is in the matter of the Superintendency of the Naval Observatory. The Army had created the Weather Service and developed it to its present proportions with relatively far less assistance from professional men of science, than is the case with the Naval Observatory.

There is no other country in the world of great importance which has not at least one National Observatory under a responsible scientific management, and the director is usually a man of high astronomical attainments. The policy of the leading powers has provided for several such institu-

tions under national patronage, every one of them in charge of a professional astronomer. There is, furthermore, nowhere except in this country a Naval Observatory which pretends to any rank as an astronomical institution. Ours is the only country in the world, and in all history, in which any footing has been secured for the doctrine that a National Observatory can be more advantageously managed by men who are not astronomers, and that astronomers are by nature incapable of exercising such a trust. This charge is an imputation on every American astronomer which should be resented, for it applies, if at all, to American astronomers alone. One needs only to glance at the roll of successful directors of large national astronomical institutions in other lands to see what a grievous insult to American astronomy our system with its implication is. Not to mention the present distinguished incumbents, who are almost invariably highly competent men in every respect, we have, among others, in the list of directors of the Royal Observatory at Greenwich, Flamstead, Bradley, Maskelyne, Pond and Airy; at the Cape, Maclear and Stone; at Paris in recent times, Arago, Le Verrier and Delaunay; at Berlin such men as Encke; at Vienna, Littrow; at Poulcova, the Struves. These are but few of the names that could be mentioned to combat the ridiculous proposition that astronomers cannot successfully manage a great National Observatory,—a proposition which is, perhaps, too much dignified by serious refutation.

It is a reflection upon the astronomers of this country, so long as the present system endures,—upon the country for lack of intelligence, or upon astronomers for incompetence, or in equal measure upon both. There is no escape from this conclusion.

Dudley Observatory, Feb. 23, 1891.

THE PROPER MOTION OF Σ 1321.

S. W. BURNHAM.*

FOR THE MESSENGER.

The components of the double star, Σ 1321, constitute a system similar to 61 Cygni, but one even more interesting in

* Lick Observatory, California.

some respects, and particularly so far as the proper motion of the stars is concerned, although the movement in space is slower, and the stars much more distant as shown by the parallax observations. The parallax of the principal star of Σ 1321 is given by Kapteyn as 0″.087 (*A.N.* 2935), but this may be considered as subject to a relatively large correction, and perhaps only indicates that the star probably has a small parallax. The proper motion of this star according to Argelander is 1″.69 in the direction of 247°.5. For a long time it has been apparent from the measures of A and B that their relative motion was very small, the change in angle since the measures of Struve in 1832 being only 14° with diminution of about 0″.6 in the distance.

The original of the accompanying diagram was made on a scale of 8″ to the inch, and then reduced in the camera to the size here shown. A list of carefully selected observations by the best observers, at intervals of six or eight years, was made, and the measured angles and distances of B platted accurately to scale from the several positions of A along the line of its movement as determined by Argelander's proper motion. Thus we have a graphical representation of the two stars precisely as they were at the respective times.

The measures used are the following:

1832.96	48°.4	20″.10	Σ	3n
1840.32	50 .5	20 .18	*O*Σ	3n
1848.57	52 .2	20 .00	*O*Σ	4n
1855.17	53 .6	19 .59	De	4n
1863.12	55 .7	19 .73	De	5n
1871.24	57 .3	19 .67	Du	3n
1877.97	59 .2	19 .63	Hl	9n
1891.11	62 .4	19 .42	*β*	3n

The several positions of B fall almost exactly in line, so that there is no uncertainty as to the direction of its motion. This star is moving in the direction of 244°.5, and, therefore, makes an angle of exactly 3° with the path of A. The movement of B during this interval of nearly sixty years, as measured on this drawing, is a trifle over 0″.1 less than the assumed motion of A; and it is, therefore, safe to say that the two have the same proper motion, since the apparent difference is far below the ordinary errors of observation, and the chance of getting a less difference in stars of exactly the same proper motion would be exceedingly small.

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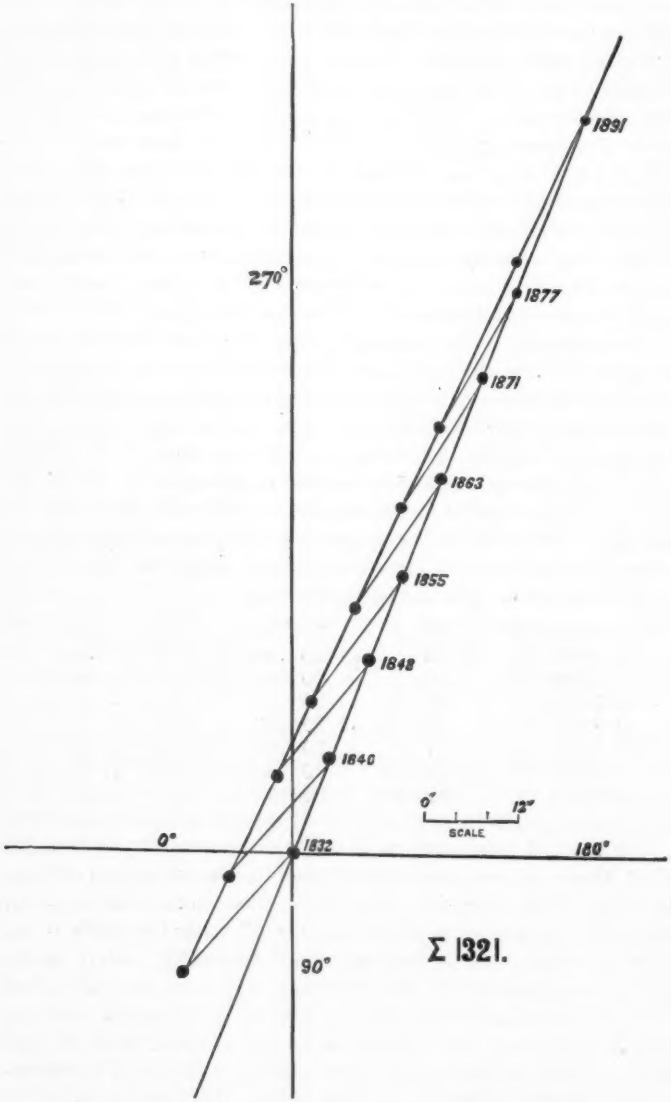

Σ 1321.

Among the stars which are apparently only optically double, we have many examples where the known proper motion of the primary is also that of the companion, so that the two stars have remained unchanged with relation to each other, but I do not recall any other instance where these motions are practically the same, but in a slightly different direction. Other examples will doubtless be found when pairs with apparently the same movement are more fully observed.

I have previously shown with deference to 61 Cygni (SID. MESS., Jan., 1891) that the paths of the two stars make with each other an angle of 2°.0, while the difference in proper motion amounts to 0″.8 annually, which is larger than any probable difference between the motions of Σ 1321 during the whole period of observation.

It will be seen from the diagram that the paths of these two stars will soon cross each other, but evidently there will be little change in the distance during the next century. The comparatively rapid change in the relative positions of the stars of 61 Cygni is the result, not of their greater proper motion, but of the inequality of the two motions.

PHENOMENA OBSERVED UPON SATURN AT THE TIME OF THE PASSAGE OF THE SUN AND OF THE EARTH THROUGH THE PLANE OF ITS RINGS, IN 1877-1878.*

E. L. TROUVELOT.†

IV. *The Anterior Part of the Ring Appears Narrower than its Posterior Part.*

We have observed another phenomenon, which, if it is not in direct relation with those which we have just made known, is nevertheless very interesting, for it indicates that there are still obscure points for us regarding the rings of Saturn. In fact, Dec. 6, 1878, we were able to prove with clearest evidence that the anterior part of the ring which passes before the globe (Fig. 7) was much narrower at A, against the eastern limb, than it was at P on the posterior part which received the shadow of the globe. At A, the ring was represented by a narrow luminous thread, perfectly

* Continued from page 125. † Observatory at Meudon, France.

defined, which had scarcely a quarter of the size which it seemed to have at P. This observation made under the most favorable atmospheric conditions was immediately represented by the drawing of which Fig. 7 is a faithful copy.

December 7, 8, and 11 the same phenomenon was again observed. On the 12th it was still visible, but the thread of light representing the anterior ring was already much less narrow than on the preceding days. On the 14th it had resumed its normal form and the ring appeared perceptibly equal in size on the two opposite parts. It is perhaps worthy of remark that from Dec. 6 to 13, one saw no trace of the nebulous ring C on the globe, and that the opening of the eastern end was completely invisible, while one could

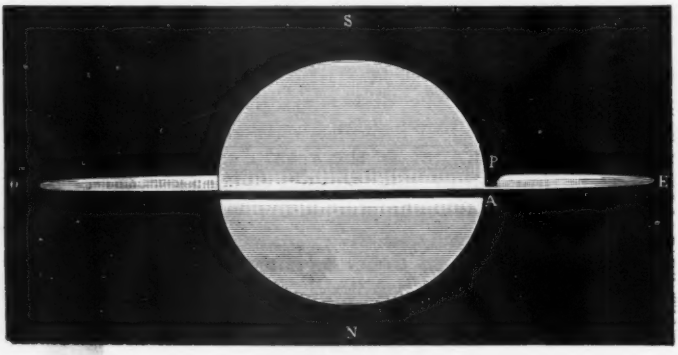

Fig. 7.

easily distinguish that of the western end. Nevertheless the opening of the eastern end, invisible the 6th, 7th, 8th, 11th, 12th and 13th of December, was on the 14th as easily seen as it was at the opposite end.

In the actual state of our knowledge of the rings of Saturn it is very difficult to explain this phenomenon. Should it be attributed to a partial and momentary super-elevation of the protuberant zone of which we have spoken, or is it indeed the result of an unknown cause which we have already had occasion to suspect? The observation does not furnish us any data for replying to these questions.

Dec. 6, 1878, the earth was at -1° 47′.3 above the plane of the rings (Fig. 1). If the phenomenon in question was due

to the momentary elevation of a part of the protuberant zone, making a screen to the more distant parts, the phenomenon must be quite rare; for, from May 1 to Aug. 15, 1877, and from Nov. 10 to Dec. 6, 1878, our globe was less elevated above the surface of the rings than at this last date, and yet the 43 observations made during these two periods revealed noting of the kind; if the phenomenon existed it passed unperceived.

V. *Disappearance of the Rings of Saturn.*

As we have shown above, the illuminated part of the ring, gradually swallowed up by the shadow, was reduced to such fine proportions Feb. 5, 1878, the evening before the passage of the sun through the plane of the rings, that it was very difficult to recognize it. Feb. 6 the planet was observed at 6 P. M.

Had the sun already passed to the south side of the rings? I do not know, but I could not see the least trace of the luminous thread observed the evening before. At this time the conditions of observation were very bad, because of the proximity of the sun and of the horizon. Yet one could readily recognize the ring on the globe where it appeared like a rather dark, grayish band.

February 7, 11, 16, 18, 19 and 27 one could distinguish with more or less ease, two short and pale triangular appendices of which the base rested on diametrically opposite points of the limb, almost in the prolongation of the dark band which represented the obscure ring crossing the planet. This sort of handle, whose light was very pale, showed a very diffuse border which it was almost impossible to catch, and which one might attribute to the light reflected across the opening of the rings. On three different occasions, February 7, 16, and 19, I thought at certain moments I could catch some traces of a feeble thread of light discontinued at each side of the planet, but the impression was so fleeting that I cannot affirm whether I was dealing with a reality or an illusion.

Saturn was again observed February 9, 13, 24 and 26, then the first of March, but under such poor conditions that I could not obtain any results in which one could place confidence. After March 1st, the day when the earth passed

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through the plane of the rings, it became impossible to observe the planet, it was so near to the sun.

VI. *The Deformation of the Limb of Saturn.*

My observations show also the deformations of the limb of Saturn which were sometimes very apparent. For instance, on the 8th of May, 1877, the edge appeared quite strongly flattened on its southeastern part, which formed a straight line instead of a curve. Jan. 30, 1878, a similar flattening was seen on the south-southeast. Dec. 19, 1880, the limb seemed much flattened on all that part which is contiguous to the shadow of the globe on the rings. Jan. 25, 1878, the limb was much flattened in the polar regions especially the southern one.

Aside from these accidental and temporary deformations of the limb, the globe of Saturn seems also subjected to changes of form which last a longer or shorter time. In any case this globe has not the form of a regular ellipse, but rather that of a flattened spheroid. Feb. 5, 1878, when the ring, almost invisible, did not inconvenience the observation at all, it became evident that the globe of Saturn is a special spehroid of which Fig. 2 gives an idea. It is the "square shouldered" form of English observers, which is a curve with fine face. The real form of Saturn can not be discovered, of course, except toward the time when the earth is very near the plane of the rings, for the higher it goes the more does the form of the planet approach a circular figure.

For this same reason it is toward the time of the passage of our globe through the plane that we should measure comparatively the polar and equatorial diameters to obtain the flattening.

From these observations the conclusion is that we sometimes see on the limb of Saturn deformations which at times appear considerable; these deformations, which are always temporary, may be real or apparent. Any cause which would darken a certain extent of the materials which compose the edge of the planet would produce a deformation apparent and not real, like that which would result from the temporary displacement of the matter composing the limb. Although we see nothing which can be opposed to one of

these explanations more than to the other, yet we are rather inclined in favor of the first, which seems to us most probable, in which the deformation is apparent and not real. In fact, we have sometimes observed deformations quite remarkable, Oct. 28, Nov. 11, 16, 18, 19 and Dec. 28, 1880, when the southern polar cap presented itself to our sight under the characteristic form of a very flat cone, and it was only after several observations of the same phenomenon that we were able to convince ourselves that this conical form was only apparent and due to the darkening of the two arcs reaching out to the limb.

VII. *The disc of Saturn has not a Uniform Luminous Intensity.*

The opinion already expressed that the globe of Saturn, unlike that of Jupiter, shines with greater brilliancy toward its periphery than toward its more central parts, has not been confirmed by our observations. In fact, while these observations do not once mention that the edge of Saturn is more luminous than the other parts of the disc, they show, on the contrary, that on Sept. 25, Dec. 18, 28 and 29, 1877, as well as Oct. 28, Nov. 24, and 26 and Dec. 6, 14, 22 and 28, 1878, this edge like that of Jupiter, was notably less luminous than the other parts of the globe.

These observations show also that on Sept. 25, 1877, Oct. 28, Nov. 24 and Dec. 12, 22 and 28, 1878, the half of the limb most distant from the sun appeared considerably darker than that which was turned toward the sun. If the last phenomenon did not indicate the beginning of a phase, it was at least the forerunner, and probably was nothing else but the kind of penumbra which on the planets always accompanies their phase. The observation of Sept. 25, 1877, was made only 17 days after the opposition of Saturn, while that of Oct. 28, 1878, was made 38 days after the following opposition; also the darkening of the penumbra was much less pronounced, especially for the second date, than it was on Nov. 24 and Dec. 14, 22 and 28 when the planet was much nearer quadrature, where the phases become the most accentuated.

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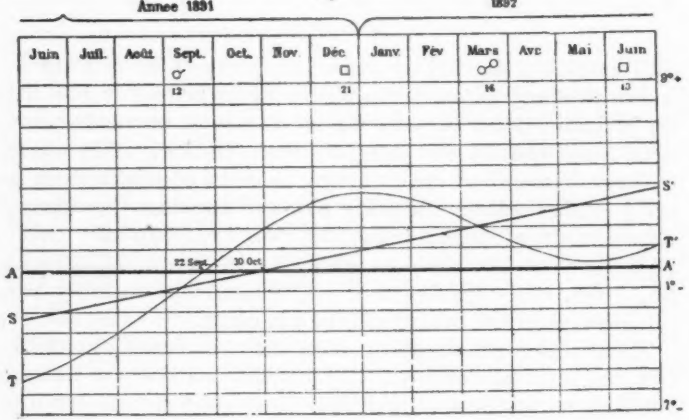
VIII. *Remarks and Suggestions on the Observations to be made on Saturn in 1891-1892.*

The approaching passage of the earth and the sun through the plane of the rings will not take place under conditions the most favorable for the delicate operations which relate to the simultaneous observation of the protuberant belts belonging to the north and south surfaces of the ring, for Saturn will be at that time too near the sun, and aside from that the passages of the two bodies are too distant the one from the other.

Fig. 8



Apparent path of the sun and of the earth with reference to the plane of the rings. AA' plane of the rings; SS' position of sun; TT' position of earth.

In examining the diagram (Fig. 8), one can easily see that the protuberant belt of the ring B, if, as we have supposed, it is true that this belt exists, cannot show simultaneously its opposite sides in 1891 because of the opaque screen formed by the ring A, which will prevent the solar rays from lighting one of them and consequently it will remain invisible. In fact, when, toward the middle of September, the elevation of the earth will not be more than $-0^\circ\ 20'$, the opposite surface as well as its protuberant zone will both be still in darkness, the height of the sun at that date being

still — 0° 42′. Sept. 22, when our globe will cross the plane of the rings, the sun will be 0° 35′ south of the ring, a height more than sufficient for the ring A to intercept the sun's rays and prevent the zone from receiving them. It will be just about the same after the passage of the earth to the north of the plane of the ring.

When the sun will have lowered on the southern surface enough to lighten the northern protuberant belt, the earth will be too much elevated above the northern surface to allow anyone to see the southern belt hidden by the screen A.

On the other hand, when the sun in its turn passes through the plane of the rings, Oct. 30, the earth will be much too high, about 2°, for the protuberant part B to be visible. There is then little probability that this interesting phenomenon can be proved in 1891 unless the shadow of the ring, enlarged by the protuberant zone will permit us to arrive at the same result when the sun has become very oblique.

But it will not be the same for the other phenomena, which we have explained in sections one, two and three which can be examined at leisure before and after the crossing of the two bodies through the plane of the rings, although the proximity of the sun may inconvenience the observations somewhat, especially at the time when they offer the greatest interest, *i. e.*, at the moment when the illuminated surface will become very small.

The phenomenon of the swallowing up of the illuminated part of the ring by a shadow can be observed from the middle of July to the beginning of September; for after the 10th, the elevation of the earth above the southern surface of the ring becoming inferior to that of the sun, the shadow will become invisible to the observer until Oct. 30, the day of the passage of the sun through the plane of the rings. After this last date one can observe at leisure the retreat of the shadow on the ring and the gradual enlargement of the lighted surface until toward the middle of February 1892. As to the decrease and increase of brightness of the opposite surfaces of the ring, the southern surface can be observed from Jan. 13 to Sept. 22, 1891; and the northern from Nov. 1 of the same year to Aug. 21, 1892. In the months of

April, May and June 1892, the earth and the sun will be situated, with regard to the northern surface of the ring, very nearly as they were for the southern surface in 1878 (Fig. 1) that is to say, at this time our globe approaches very near the plane of the ring, while the sun is at quite a distance from it. One could at this time ascertain whether phenomena of the same kind as those which were observed Dec. 6, 7, 8, 11, and 12, 1878, are visible. The conditions will be the most favorable possible for this kind of observations, in the first place because Saturn will be near quadrature, the time when the shadow of the globe on the ring and that of the ring on the globe reach their maximum of development, and also because, in 1892, our globe will approach much nearer the ring than in 1878. In fact, while on the 6th of December

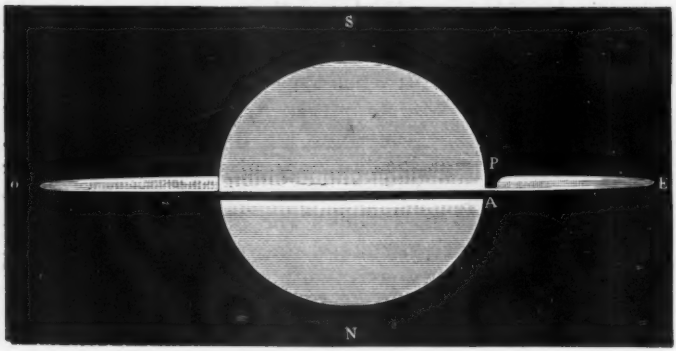


Fig. 9.

the earth was situated at $-1^\circ\ 47'.3$, on the 20th of May, 1892, it will be at $+\ 0^\circ\ 22'.2$; only, on May 20, the time when our globe will be the nearest to the surface of the ring, the shadow cast by the latter upon the globe will be perhaps a little too much separated from the exterior edge of the ring A; which condition would be less favorable for judging of the size of the anterior part of this ring. In any case, the observations made in the month of May 1892 can not fail to be extremely interesting, for the eye of the observer skimming over the surface of the rings, lighted then by a sun elevated nearly 3°, cannot fail to notice very interesting things about the structure of the rings as well as con-

cerning the cause of the encroaching shadow which we observed in 1877-1878.

Fig. 9 is a projection of the rings and represents them as they ought to appear May 20, 1892 if their surface were flat.

In order to observe with profit, toward the time of the passages of the sun and the earth through the plane of the rings, we advise observers to prepare in advance projections of the rings of Saturn according to the apparent elements of the rings given in the *Nautical Almanac* for the date of each contemplated observation; in this way it will be easy to know whether the observation agrees with the projection. Since certain phenomena can be seen in broad daylight, Saturn can be observed at the most favorable moment of the day when there is any advantage in doing it.

THE ORIGIN OF THE STELLAR SYSTEMS.

T. J. J. SEE.*

FOR THE MESSENGER.

In the February number of THE MESSENGER I have pointed out what I am convinced is the physical meaning of the high excentricities of the stellar orbits. The mathematical investigation therein referred to seems to show that tidal friction is an amply sufficient cause to account for the great excentricities, but to complete the syllogism we ought to show that no other possible cause can be assigned as the source of the observed phenomena. The array of multitudinous resources revealed in the Cosmos is such that in general it is difficult to set rigorous bounds to the possibilities of nature. Fortunately, however, in this particular case the circumstances surrounding the question at issue are such as to justify the belief that in general no other cause than tidal friction can be assigned to account for the excentricities observed in the binary orbits. Some astronomers may conceive that the excentricities indicate the fortuitous origin of these systems from separate stars. A little reflection, however, will make it clear that such an hypothesis is

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untenable. For such a chance orbit would be in general hyperbolic; and it is easy to show that should the orbit in some cases even be elliptic, its absolute dimensions would very far exceed those of binary orbits hitherto computed. Professor Lockyer in his "Meteoritic Hypothesis" has suggested that double stars originate through the approach of separate swarms of meteorites. But this suggestion, to my mind, is even more unsatisfactory than the foregoing, since it not only encounters the same difficulties, but must in addition explain how the smaller swarm would escape disintegration under the action of the chief swarm. For the smaller swarm would enter on an orbit highly elliptic or hyperbolic. In case the orbit is elliptic the disintegrating action of the larger swarm, in a comparatively short time, will scatter the meteorites of the smaller swarm along the whole length of its orbit—reduce it [as the sun reduces a comet] to a train of meteoric dust. Moreover, the observed excentricities of the stellar orbits are in general hardly sufficiently high to support this hypothesis.

Again, it has been suggested that an explosion in the process of separation has rendered the orbit of the detached body so highly excentric. This suggestion of explosions is purely an hypothesis, and we are not able to affirm that such phenomena really take place, except probably in our sun. But granting that they really take place in nebulæ, it seems very difficult to see how such explosive action could divide a nebula into two nearly equal masses. It seems more probable that the meteorites of a nebula under such conditions would merely be scattered in every direction, and that the parts would almost immediately fall back together. But even should the detached parts not immediately reunite to the largest remaining body they would revolve around it as meteoric swarms in excentric orbits, and it is impossible to conceive how, under the disintegrating action of the chief mass, they could ever be gathered into individual small bodies, much less could they be agglomerated into a second large mass.

All the foregoing hypotheses seem, therefore, to encounter fatal objections. I will now venture an hypothesis which demands nothing extraordinary, and rests, moreover, on a dynamical basis.

A homogeneous fluid sphere devoid of rotation is always in equilibrium, however its particles may be arranged with respect to one another. If this sphere be endowed with a slow rotation about any axis, it becomes a spheroid; here again we may suppose the particles exchanged with one another without any disturbance of the equilibrium. We may regard this homogeneous spheroid as made up of infinitely thin layers, all having the same ellipticity. If the rotation of the spheroid is quickened its ellipticity increases, and the ellipticities of all the strata increase equally rapid. The particles of a heterogeneous fluid sphere devoid of rotation are in equilibrium when and only when they are so arranged that the density decreases from the center to the surface. Such a sphere is composed of successive infinitely thin layers of a uniform density, but the layers decrease in density from the center to the surface.

If now the sphere be endowed with a slow rotation, it passes into a spheroid, and all its layers become spheroidal. But in this case of heterogeneity the layers do not, as in the case of homogeneity, retain the same ellipticity in passing from the center to the surface. The ellipticities increase, as is mathematically established by Laplace in the second volume of the *Mecanique Celeste*. Thus the ellipticities of the layers of a heterogeneous fluid spheroid in equilibrium follow a law the reverse of the densities. Sir John Herschel has observed that certain nebulæ actually appear to confirm this law. Since the ellipticities were zero when the body was devoid of rotation, it is clear that acceleration of axial velocity will cause them all to increase until a certain limit is reached. But all the ellipticities will not increase equally rapid; the ellipticities of the outer, less dense, layers will gain ever more and more rapidly upon the denser and less elliptic layers at the center. The divergence in the ellipticities thus increases as the rotation proceeds. With a given velocity of rotation the divergence increases with increase of heterogeneity. This being established, let us now consider some other figures of equilibrium. Throughout this discussion we are supposing the law of attraction to be Newtonian gravitation. In the figures of equilibrium hitherto determined, owing to mathematical difficulties at present insurmountable, nearly all investigators have pre-

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supposed the fluid to be homogeneous. Among the figures established on this hypothesis is the Jacobian ellipsoid of three unequal axes; and the spheroid may be regarded as a particular case where two of the axes of the ellipsoid are equal. This Jacobian ellipsoid set rotating in equilibrium about its shortest axis is rigorously stable.

M. Poincaré has discussed the dynamics of the Jacobian ellipsoid with particular care [*Acta Math.* Vol. VII]. He finds that as the rotational velocity of the ellipsoid increases the ellipsoid gradually flattens out, and finally passes into the accompanying very remarkable figure.

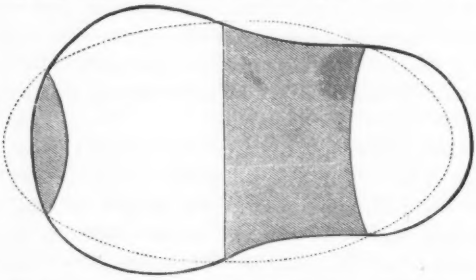

FIG. 1. SECTION OF M. POINCARÉ'S FIGURE OF EQUILIBRIUM. THE SHADED PORTION FALLS WITHIN THE ELLIPSOID.

Fig. 1 represents a section of M. Poincaré's figure through the shortest and longest axes (corresponding respectively to similar axes in the ellipsoid). This furrowing of the Jacobian ellipsoid parallel to its smallest section indicates, as M. Poincaré points out, a tendence of the body to split up into two comparable masses. At the stage represented in Fig. 1 the equilibrium is rigorously stable. M. Poincaré expresses disappointment in the conclusion of his excellent memoir that his investigation did not throw much light upon Laplace's Nebular Hypothesis. Professor Darwin undertook a somewhat similar investigation independently, and completed it after the publication of M. Poincaré's paper. He sought to determine the figure which two homogeneous spherical fluid masses would assume under their mutual attraction when set in synchronistic orbital motion about their common center of gravity so close together that their surfaces nearly touched. His result is given in Fig. 2—equal masses.

In Fig. 2 the whole sections are given—one through the axis of rotation, the other perpendicular to it. For details and an admirable discussion of this remarkable dumb-bell figure of equilibrium, see *Phil. Trans.* 1887.

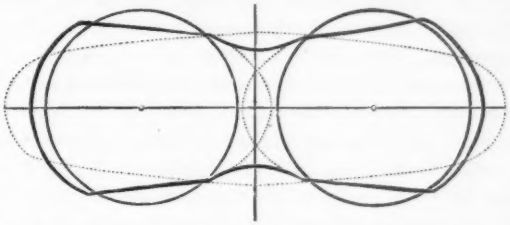

FIG. 2 (*a*). SECTION OF MR. DARWIN'S DUMBBELL FIGURE OF EQUILIBRIUM PERPENDICULAR TO AXIS OF ROTATION. DOTTED SECTION JACOBIAN ELLIPSOID OF SAME MASS AND MOMENT OF MOMENTUM. SCALE ONE-THIRD OF THE ORIGINAL.

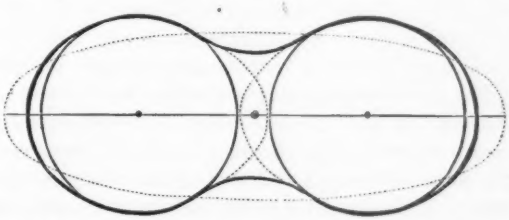

FIG. 2 (*b*). SECTION THROUGH AXIS OF ROTATION.

At the conclusion of his work Mr. Darwin, like M. Poincaré, remarks that but little help is gained towards understanding Laplace's Hypothesis. He adds that both his results and those of M. Poincaré's seem to show that the mass separated should bear a very much larger ratio to the parent mass than is observed in the planets and satellites of the solar system. He states that the mass separated can not bear a less ratio [in case of homogeneity] than about one-thirtieth. He agrees with M. Poincaré that when the equilibrium of the Jacobian ellipsoid breaks down, the body splits up into two comparable masses.

Now let us examine the significance of these mathematical results in our study of the origin of the binary stars. Before we proceed further let the reader compare the above figures with Sir John Herschel's drawings of double nebulæ,

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published in *Phil. Trans.* for 1833. Compare M. Poincaré's figure with the figures numbered by Sir John Herschel respectively 68, 69, 70, 71, 72, 73, 74 [representing the nebulæ numbered: 1252, 1202, 604, 1146, 444, 2197, 1408]; and I think it will be impossible to escape the conclusion that the foregoing mathematical results are exemplified in the heavens. Nebulæ absolutely homogeneous of course do not exist; yet there are abundant reasons for thinking that in many cases the departure from homogeneity is not very marked. If the nebula is not heterogenous to a considerable degree we may take the figures of M. Poincaré and Professor Darwin to represent substantially what would occur as gravitational contraction proceeds. And we may consider the figure of a nebulae undergoing development momentarily, but not secularly, stable, owing to the continual dissipation of its energy and the contraction consequent therefrom. Hence we see that if the nebula be homogeneous the mass-ratio which will obtain between its parts when it splits up will be a very large fraction, perhaps one-half, one-third, or one-fourth, or smaller, and possibly under certain conditions unity. The above mathematical investigations seem to show that in general the masses would not be exactly equal, but always comparable—*i. e.* always of the same order. If the heterogeneity be not very considerable, when the parent nebula splits up, this same relative mass-ratio will be nearly maintained. There is reason to believe that many of the nebulæ are not very heterogeneous, and we ought therefore to expect a separation into comparable masses. This theoretic inference accords with the great number of double nebulæ and double stars actually observed in the heavens. The greater the heterogeneity the greater the divergence in mass-ratio from that obtaining in case of homogeneity. This clearly follows from what we have said elsewhere concerning the concentration of density about the center of the parent nebula, and the increase in the ellipticities of the strata as we pass to the surface. For the part thrown off will be taken chiefly from the peripheral regions of the parent mass; and even should the volume detached retain the same ratio to the parent volume as in the case of homogeneity, still the mass would bear a much smaller ratio to the parent mass, owing to the inferior density of

the part detached. But in fact it is easy to see that both the mass and the volume split off will be less in case of heterogeneity. If the nebula be very heterogeneous the part detached ought to bear a very small mass-ratio to the central body. With respect to the planetary system this conclusion would seem to indicate that when the planets were formed our original nebula was very heterogeneous. Under this hypothesis our remarkable system is seen to be a particular case—an exception to the general process of cosmic evolution by which nebulæ develop first into double nebulæ, and then into systems of binary stars. Thus far the mass-ratio of the components of stellar systems has been determined satisfactorily from measurements in but few cases. But if we take the binary stars as a class we shall not err greatly in supposing the mass-ratio to correspond with the amount of light we receive, and this can be determined from the magnitudes of the stars.

In particular cases this method of inferring the relative masses of stars of a system from their magnitudes would doubtless fail. And in general the value thus deduced would probably be a little too small; still, it would at least give a rough approximation to the truth. It now remains to examine the orbit which the detached mass would pursue when first split off. From the very nature of nebular contraction we may with safety assume that acceleration in angular velocity proceeds slowly and steadily—there are no sudden jerks or stops. This being the case it is certain that separation takes place very slowly; and we may unhesitatingly affirm that in general many revolutions are performed while the splitting process is in progress. Sudden acceleration in angular velocity being impossible, we see that the orbit the detached mass begins to pursue must be nearly circular. Were the orbit highly excentric the action of the larger body would soon reduce the smaller mass to a train of meteoric dust; and when once strewn out into a girdle it could never condense into a star. But the mass split off being set revolving in an orbit nearly circular, the action of the larger body could not spread the smaller swarm out into a meteoric ring; and, condensation forthwith proceeding, it would escape disintegration when the orbit eventually became expanded and highly excentric from the secu-

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lar effects of tidal friction. When just separated [as is apparently the case in Herschel's figure 68] the masses doubtless rotate and revolve in synchronism. But the dissipation of the energy of either mass soon permits gravitational contraction, and this causes the axial angular velocity to surpass the orbital angular velocity, and then tidal friction begins to operate. Under these conditions tidal friction increases both the major axis and the excentricity of the orbit. And in general these elements will continue to increase until the rotations of both stars are reduced to synchronism with the orbital revolution. When this state is reached, we have the maximum of excentricity; afterwards the excentricity is again very slowly reduced to zero by the secular action of libration. Such a rigid state would probably not be reached until both bodies have become entirely dark, since the angular velocity will continue to accelerate as long as contraction takes place. When finally this state is reached, however, the stars will show always the same faces to each other, and the system will continue to revolve as though rigidly connected forever, unless acted upon by external forces, or ethereal or meteoric resistance. These suggestions of secular dynamic instability have a particular interest also when applied to systems of the Algol type, where the bodies are not only both very large, but also very close together, and therefore the tides in all probability enormous. From what we have said it will appear that astronomers have hitherto greatly underestimated the importance of tidal friction as a formative agency in the history of the universe. It has certainly left a distinct impress upon the orbits of the stellar systems, and even in the case of the solar system it has exercised some influence. I do not therefore hesitate to rank tidal friction as one of the most important agencies operative in the development of the various systems of the Cosmos.

In conclusion I beg to add that this and my former article constitute only a very brief outline of the general conclusions at which I have arrived by the investigation mentioned. An investigation which explains satisfactorily the great excentricities of the stellar orbits, the large mass-ratio of the components, and intimately connects binary systems with double nebulæ, constitutes a coherent theory, of which the probability, to my mind, closely approaches certainty.

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It is diffiicult to see how either M. Poincaré or Professor Darwin could have felt disappointed, had he realized the full significance of his splendid dynamical researches as applied in the Universe at large, and not merely as reflected by the solar system. It appears to me that more light will be thrown upon the general process of cosmic development, if, instead of studying the Universe in general through the solar system, we base our general deductions on a study of the double nebulæ and stellar systems. Thus we may hope eventually not only to attain to a safer scientific basis for stellar cosmogony, but we may also even approach the formation of the solar system from anew point of view—from the sidereal heavens. On first sight it appears doubtful whether the planets and satellites were separated, as Laplace, supposed, in the form of rings; a separation in the form of lumps or bulky masses seems in general more probable. Finally, whatever may prove to be the value of the ideas I have herewith advanced, it is but proper to state that whilst I began the investigation and arrived at the main conclusions independently, nevertheless in the exact mathematical investigation I have profited by a study of Professor Darwin's extensive tidal researches, and therefore I should be ungracious, if not, indeed, in some measure unjust, if I did not acknowledge my especial indebtedness to Professor Darwin for some methods of research employed in his excellent memoirs.

UNIVERSITY, BERLIN, PRUSSIA,
February 7th, 1891.

THE SPIRIT LEVEL.

A. HALL.*

FOR THE MESSENGER.

In connection with the interesting remarks of Professor Safford on observations with meridian circles I should like to call attention to the use of the spirit level.

In the description of the prime vertical instrument at Pulkowa W. Struve estimates the probable error of a single level determination as $\pm$ 0″.02. On beginning observations

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with a similar instrument at the Naval Observatory thirty years ago it appears to have been assumed that this estimate was correct, and that the error of level could be perfectly corrected by means of the spirit level. The level was investigated in 1862, and again at the close of the work in 1867; and apparently it was an excellent instrument. On reducing the observations the results from the single wires on any day were generally in good agreement, but the results for different days were more discordant than one would expect. Discordances of this kind, however, are usual, but in this case they led me to suspect that the level had not always given the true position of the axis of rotation. This idea has been confirmed by the subsequent examination and use of a number of spirit levels, and I now think that the accuracy ascribed to this instrument has been exaggerated. It is only occasionally, I think, that one finds a spirit level in which the motion of the bubble corresponds exactly and readily to a small change of level. We ought not therefore to investigate a spirit level on a certain date, and at a certain temperature, and then assume that its action remains the same for a long time, and for all temperatures. I am inclined to question the practice of putting a level in a certain position and then to continue readings without touching it. It would be better to subject the level to a variety of changes. In fact this instrument should be used like any other, with the idea of eliminating constant errors that can arise from any defect or roughness that may cause the bubble to stick at a given position. It would hardly be worth while to observe the declination of a star on certain divisions of a circle for many years without examining the errors of graduation, and it would be better in any case to shift the circle so as to bring new conditions to bear in the determination of the star's position. This is only a step toward transferring constant errors to the class of accidental ones, something that should always be done if possible.

An example of trusting too much to the spirit level may be seen in the new determination of longitude between Paris and Greenwich.

March 10, 1891.

NEW METHOD FOR THE SIMULTANEOUS DETERMINATION OF LATITUDE AND AZIMUTH.

JOSEPH S. CORTI.*

In this method we measure the difference between the azimuths of *at least two stars* when they are at their greater elongation, and we compute with that value and the declinations of both stars, the azimuth of each of them and the observer's latitude.

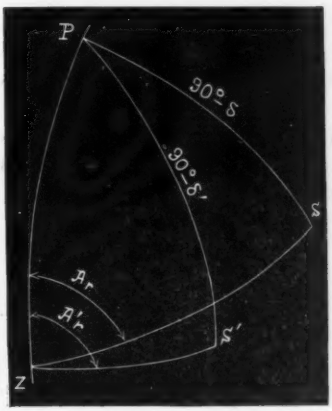
In order to get at the final formulæ let us consider separately the two following cases:

I. Both stars are on the same side of the meridian at their greater elongation.

II. One star is on one side of the meridian, and the other on the opposite one.

Case I.

Let PZ be the meridian, S the position of one of the stars, of declination $\delta = 90° - PS$, at its greatest elongation PS its hourly circle and ZS its vertical circle.



Calling A_p the angle made by the vertical circle of the star with the polar side of the meridian; then $PZS = A_p$.

Also for the star S',

$PS' = 90° - \delta'$; $PZS' = A'_p$.

And $ZP = 90° - \varphi$, φ being the latitude of the observer's place.

The spherical triangles ZPS' and ZPS have their angles S' and S right; then:

$$\sin A'_p = \frac{\cos \delta'}{\cos \varphi} \qquad (1)$$

$$\sin A_p = \frac{\cos \delta}{\cos \varphi} \qquad (2)$$

Subtracting (2) from (1)

$$\sin A'_p - \sin A_p = \frac{\cos \delta' - \cos \delta}{\cos \varphi} \qquad (3)$$

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NOTE.—Ar and A'r in both cuts should read A_p and A'_p.

adding (2) to (1)

$$\sin A'_p + \sin A_p = \frac{\cos \delta' + \cos \delta}{\cos \varphi} \qquad (4)$$

and, finally, dividing (3) by (4)

$$\frac{\sin A'_p - \sin A_p}{\sin A'_p + \sin A_p} = \frac{\cos \delta' - \cos \delta}{\cos \delta' + \cos \delta}$$

Changing the sums and differences of sines and cosines into products in the last formula, we have:

$$\frac{2. \sin \frac{A'_p - A_p}{2} \cos \frac{A'_p + A_p}{2}}{2. \sin \frac{A'_p + A_p}{2} \cos \frac{A'_p - A_p}{2}} = \frac{-2. \sin \frac{\delta' + \delta}{2} \sin \frac{\delta' - \delta}{2}}{2. \cos \frac{\delta' + \delta}{2} \cos \frac{\delta' - \delta}{2}}$$

$$\therefore \frac{\operatorname{tg} \frac{A'_p - A_p}{2}}{\operatorname{tg} \frac{A'_p + A_p}{2}} = -\operatorname{tg} \frac{\delta' + \delta}{2} \operatorname{tg} \frac{\delta' - \delta}{2} \qquad (5)$$

Knowing the declinations of both stars the second number of (5) will be known and one of the quantities

$$\frac{A'_p - A_p}{2} \text{ or } \frac{A'_p + A_p}{2}$$

may be computed, should the other one be had by measurement.

In our Case I, the angle SZS', that is, the difference between the instrumental azimuths of the observed stars, is just the value of $A'_p - A_p$, then from (5)

$$\operatorname{tg} \frac{A'_p + A_p}{2} = -\operatorname{tg} \frac{A'_p - A_p}{2} \operatorname{cotg} \frac{\delta' + \delta}{2} \operatorname{cotg} \frac{\delta' - \delta}{2} \qquad (6)$$

after which we may compute $\frac{A_p + A_p}{2}$.

Thence:

$$\left.\begin{aligned} A'_p &= \frac{A'_p + A_p}{2} + \frac{A'_p - A_p}{2} \\ A_p &= \frac{A'_p + A_p}{2} - \frac{A'_p - A_p}{2} \end{aligned}\right\} \qquad (7)$$

With each of these two values we may compute the latitude by:

$$\cos \varphi = \frac{\cos \delta}{\sin A_p} \text{ or } \cos \varphi = \frac{\cos \delta'}{\sin A'_p} \qquad (8)$$

Before considering Case II let us find the most favorable circumstance for the determination of $\frac{A'_p + A_p}{2}$ by (6) and for that of φ by (8).

Taking to simplify:

$$\Sigma = \frac{A'_p + A_p}{2} \text{ and } \Delta = \frac{A'_p - A_p}{2},$$

e. g. (6) may then be written down

$$\operatorname{tg} \Sigma = - \operatorname{tg} \Delta \operatorname{cotg} \frac{\delta' + \delta}{2} \operatorname{cotg} \frac{\delta' - \delta}{2}.$$

An error whatever on Δ will give an error on Σ, but no doubt, it will do nothing to $\frac{\delta' + \delta}{2}$ nor to $\frac{\delta' - \delta}{2}$; then in the last formula the two contangents will be constants, and Δ and Σ variables depending one from the other.

By differentiation of the last formula we have

$$\frac{d\Sigma}{\cos^2 \Sigma} = \frac{- \operatorname{cotg} \frac{\delta' + \delta}{2} \operatorname{cotg} \frac{\delta' - \delta}{2}}{\cos^2 \Delta} . d\Delta,$$

$$\therefore d\Sigma = - \frac{\cos^2 \Sigma}{\cos^2 \Delta} \operatorname{cotg} \frac{\delta' + \delta}{2} \operatorname{cotg} \frac{\delta' - \delta}{2} d\Delta,$$

Which shows that an error whatever, $d\Delta$ on the difference between the instrumental azimuths of the observed stars, will give us on Σ an error equal to the former multiplied by a factor

$$- \frac{\cos^2 \Sigma}{\cos^2 \Delta} \operatorname{cotg} \frac{\delta' + \delta}{2} \operatorname{cotg} \frac{\delta' - \delta}{2};$$

and as this factor will be smaller, the *greater* the difference $\delta' - \delta$, then the most favorable circumstance for the computation of Σ is that in which

$$\delta' - \delta = \text{maximum} \qquad (9)$$

The value of A_p once found, *e. g.* (8) will give us the latitude by

$$\cos \varphi = \frac{\cos \delta}{\sin A_p}.$$

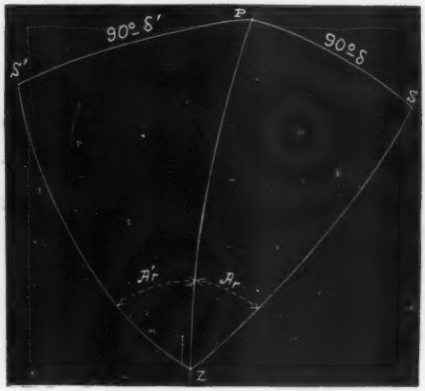
Differentiating this equation in which A_p and φ are variables and δ constant,

$$- \sin \varphi \, d\varphi = \frac{- \cos \delta \cos A_p}{\sin^2 A_p} . dA_p$$

$$\therefore d\varphi = \frac{\cos \delta}{\sin \varphi} \operatorname{cotg} A_p \operatorname{cosec} A_p . d A_p,$$

which shows that an error on A_p gives on φ an error so much smaller, the farther the star will be from the meridian

at the moment of its greatest elongation, that is to say, for a given place the best star will be the nearest one to the equator.



CASE II.

Let, as before, P and Z be the celestial pole and the zenith, S and S′ the stars observed at their greater elongation.

If δ, δ' and A_p, A'_p be the declinations of the stars and the angles made by their verticals with the polar side of the meridian, then

$$PS = 90° - \delta; \quad PS' = 90° - \delta';$$
$$SZP = A_p; \quad S'ZP = A'_p;$$

and also $PZ = 90° - \varphi$, φ being the latitude of the observer's place.

The spherical triangles SZP and S′ZP, rectangles at S and S′ will give

$$\frac{\operatorname{tg}\frac{A'_p - A_p}{2}}{\operatorname{tg}\frac{A'_p + A_p}{2}} = -\operatorname{tg}\frac{\delta' + \delta}{2}\operatorname{tg}\frac{\delta' - \delta}{2} \qquad (5)$$

In this case angle S′ZS = difference between instrumental azimuths of the observed stars $= A'_p + A_p$; then from (5)

$$\operatorname{tg}\frac{A'_p - A_p}{2} = -\operatorname{tg}\frac{A'_p + A_p}{2}\operatorname{tg}\frac{\delta' + \delta}{2}\operatorname{tg}\frac{\delta' - \delta}{2} \qquad (10).$$

from which we will get the values of A_p and A'_p by (7), and φ by (8)

Let us find for this case also the most favorable circumstance for the computation of $\frac{A'_p - A_p}{2}$ by (10).

Taking, as in case I, $\Sigma = \frac{A'_p + A_p}{2}$ and $\Delta = \frac{A'_p - A_p}{2}$, *e. g.* (10) may be written down

$$\operatorname{tg}\Delta = -\operatorname{tg}\Sigma\operatorname{tg}\frac{\delta' + \delta}{2}\operatorname{tg}\frac{\delta' - \delta}{2},$$

where the only variables are Σ and Δ.

Differentiating

$$\frac{d\Delta}{\cos^2 \Delta} = -\frac{d\Sigma}{\cos^2 \Sigma} \operatorname{tg}\frac{\delta' + \delta}{2} \operatorname{tg}\frac{\delta' - \delta}{2}$$

$$\therefore d\Delta = -\frac{\cos^2 \Delta}{\cos^2 \Sigma} \operatorname{tg}\frac{\delta' + \delta}{2} \operatorname{tg}\frac{\delta' - \delta}{2} \cdot d\Sigma,$$

which shows that an error whatever, $d\Sigma$, on the difference between the instrumental azimuth of the observed stars, will give on Δ an error equal to it multiplied by a factor

$$-\frac{\cos^2 \Delta}{\cos^2 \Sigma} \operatorname{tg}\frac{\delta' + \delta}{2} \operatorname{tg}\frac{\delta' - \delta}{2}.$$

and as this factor will be smaller, the *smaller* the difference $\delta' - \delta$, then the most favorable circumstance for the determination of Δ is that in which

$$\delta' - \delta = \text{minimum.} \qquad (11)$$

EXAMPLE.

At San Juan (Argentine Rebublic) we have observed three stars at their greatest elongation, the 1st of May of the present year, with the following readings observed by a 6 in. theodolite reading to 10″ of arc:

EAST OF THE MERIDIAN.

Star.	Declin.	Instrum. reading.
β Triangulis Australis:	− 63° 5′ 25″;	230° 9′ 55″
α Triangulis Australis:	− 68° 49′ 22″;	237° 9′ 42″.5

WEST OF THE MERIDIAN.

α Navis (Canopus):	− 52° 38′ 25″;	307° 37′ 57″.5
Fixed staff;	Instrumental reading 79° 11′ 30″	

To compute the azimuth we may arrange the following pairs of observations:

1st. α and β Triangulis Australis on the same side of meridian (Case I).

2d. α Triangulis Australis to the east and Canopus to the west (Case II).

3d. β Triangulis Australis to the east and Canopus to the west (Case II).

We do not take into account the 1st pair, for its data do not satisfy condition (9), and computing (10), (7) and (8) for the other two, we have what follows:

Value of A_p:		
by 2d pair for α T. A.	25° 4′ 38″.2	
" " " " Canopus		45° 23′ 36″.8
" 3d " " β T. A.	32° 4′ 23″.2	
" " " " Canopus		45° 23′ 39″.1
and taking the mean for Canopus		45° 23′ 37″.9

Starting from these values the azimuth of the fixed staff will be

by α Trian. Austr.	3° 2′ 50″.7
" β " "	3° 2′ 45″.3
" Canopus	3° 2′ 49″.6

mean: $A = 3° 2′ 48″.5$

and the latitude

by α Trian. Austr.	$-31° 32′ 0″.5$
" β " "	$-31° 32′ 4″.2$
" Canopus	$-32° 31′ 2″.2$

mean: $\phi = -31° 32′ 2″.3$

Should we have not rejected the 1st pair of observations, we would have had two values of A_p for each star, and taking the mean for each, the computation would have given

$$A = 3° 2′ 56″.8; \quad \phi = -31° 32′ 12″.8.$$

ON THE ORIGIN OF GAPS IN THE ZONE OF ASTEROIDS.

DANIEL KIRKWOOD.

FOR THE MESSENGER.

In regard to the physical constitution of the asteroid zone between Mars and Jupiter it is known by astronomers—(1) that well defined gaps occur in the arrangement of the mean distances; and (2) that these blanks or chasms coincide with the respective distances at which the periods of minor planets would have a simple commensurability with the period of Jupiter. These coincidences, then, are either accidental, or the original asteroids have been eliminated by the dominating mass of Jupiter. The laws of probability compel our rejection of the former alternative.* We must therefore admit a relation of some nature between Jupiter's influence and the observed vacant spaces. What, then, was the *modus operandi* of Jupiter's power?

* The Asteroids, p. 46.

In the nebular hypothesis the sun's centrifugal force was not the only agent in separating planetary masses from the central body. The tide-producing power of previously formed planets, especially of Jupiter, was also effective. It is easy to see that the process of asteroid formation was more active in sections of commensurability than in spaces intervening. Those bodies were not only separated from the sun, in part, by the influence of the "giant planet," but again eliminated by his agency from the planetary cluster. Was the latter process a slow and gradual one—a *secular* variation, or did the maximum activity at critical epochs, issue in the sudden extinction of new members by their passing, in perihelio, beneath the solar surface? That the latter was nature's method, as we have more than once intimated,* seems, we think, not wholly improbable.

In the first stages of Jupiter's planetary existence its diameter was probably several millions of miles. It is hence not impossible that some original asteroids may have been lost in its mass. A more probable result, however, would be their re-absorption into the sun itself. Let us suppose an asteroid separated in a tidal protuberance, at the distance, say, of 4.481. Its period would be four-fifths of Jupiter's. The initial eccentricity would doubtless be considerable. If but 0.15, the present average of known asteroids, its perihelion distance would have been 3.8; in other words the point of nearest approach to the sun's centre would have been sixty million miles beneath its surface. We see, therefore, that the separate existence of the asteroid could have been but temporary. Again, let us suppose an original asteroid in the maximum chasm, with a period equal to half that of Jupiter. Its mean distance from the sun's centre would be 3.2776. With a primitive eccentricity equal to that of the earth's orbit, (0.017) the perihelion point would be 5,000,000 miles beneath the surface of the solar mass. Similar reasoning would obviously apply to other simple cases of commensurable motion. Only those asteroids whose primitive perihelion distances were greater than the radius of the contracting solar volume could permanently maintain a planetary existence. In this view of

* See the author's "Asteroids" pp. 38-47.

the subject the chasms of the zone originated with the zone itself, and were not produced by Jupiter's *secular* perturbation. It must be remarked however, that the present observed eccentricity of many asteroids could not have obtained from the beginning, as was shown by the writer in the work already quoted.*

Intimately connected with this question of the asteroids is that of the eccentricity of Jupiter's orbit; its origin; the increased intensity, in perihelio, of Jupiter's perturbation on the solar nebula, the forming asteroids, etc., etc.—all questions within the domain of physical science. More facts however, are needed for their discussion.

CURRENT CELESTIAL PHENOMENA.

THE PLANETS.

Mercury will be at greatest elongation east of the sun, 20° 01′, April 18. It will then be visible to the naked eye an hour and a half after sunset. One should look for the planet a little to the north of the west point of the horizon. The northern declination of the planet makes this month a very favorable time for daylight observations of Mercury, the best time for them being after the middle of the afternoon.

Mercury will be at the descending node of its orbit May 9, at 3^h A. M., central time, and at inferior conjunction with the sun May 9, at 8^h 41^m P. M. As this conjunction occurs so near the node, the planet will cross the disk of the sun. The transit will only be partially visible in the United States. At Washington the times of the phases will be as follows:

Ingress, exterior contact,	May 9 11^h 55^m 29^s.3	Greenwich mean time.
Ingress, interior contact,	9 12 00 25 .0	
Least distance of centers 12′ 32.4″	9 14 23 54 .5	
Egress, interior contact,	9 16 47 02 .3	
Egress, exterior contact,	9 16 52 45 .7	

The point of first contact at ingress will be 115° 31′ E., and that of last contact at egress will be 168° 15′ W. from the north point of the sun's disk.

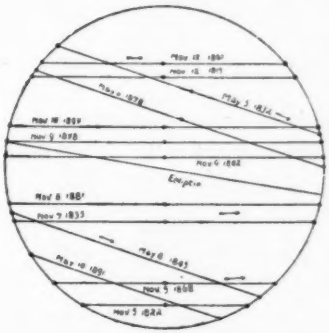
At places in the eastern part of the United States the transit will begin very near the time of sunset so that no observation of value can be made. The following are the standard times of external contact at ingress, as calculated for several of the observatories by Professor George W. Coakley (see page 40):

Observatory.	External Ingress.	Standard.
Harvard College	6^h 54^m 32^s.55	Eastern.
New York City	6 54 25 .81	"
Washington	6 54 18 .64	"
Chicago	5 54 19 .73	Central.
Carleton College	5 54 26 .68	"
Lick	3 54 17 .99	Pacific.

* Asteroids, p. 47.

For any other place the time of ingress may be found by means of the following formula, in which ρ denotes the radius of the earth at the place, the geocentric north latitude, ω the longitude *west* from Washington, and L the longitude of the nearest standard meridian from Greenwich.

$$T = 11^h\ 55^m\ 29.3^s - L + 71.66^s\ \rho \sin \phi$$
$$- 157.88^s\ \rho \cos \phi \cos (20^\circ\ 16'\ 55.5'' - \omega).$$



The time of internal contact at ingress will in every case be about five minutes later than that of exterior contact. The diameter of the planet at the time of transit will be 12.0″, so that it will be easily seen with a small telescope but will not be visible to the naked eye. The accompanying cut showing the path of Mercury across the disk of the sun during each transit in this century is copied from *L'Astronomie* for January, 1891.

Venus is "morning star" but is receding behind the sun so that she can not be readily seen. Venus and Jupiter will be in conjunction April 7, at $3^h\ 25^m$ P. M., central time, the distance between the two planets being then only 13′.

Mars may be seen in the west each evening until nine o'clock. He is now among the faint stars of Aries and will during this month move eastward into Taurus between the Pleiades and Hyades.

Jupiter is "morning star" with Venus. The conjunction of the two planets, April 7, has been mentioned above. After that date Jupiter will be the more westerly of the two, both being brighter than any of the stars in the morning sky. It is rapidly coming into good position for observation.

Saturn is on the meridian now at about ten o'clock in the evening, and doubtless most lovers of astronomy have already had many excellent views of the planet. The rings are inclined at an angle of only five degrees to the line of sight so that their width is greatly foreshortened, yet in moments of good definition one can distinguish the three rings with a telescope of moderate power. We would call especial attention to the suggestions of Professor Trouvelot (page 171) as to what observations may and should be made this year upon the ring system of Saturn.

Uranus will be at opposition April 19, rising then at about sunset. The best hours for observing this planet will be from ten to two o'clock when it will be not far from the meridian at an altitude of about 35°. We refer those who wish to look it up with small telescopes to the diagram in our last number (page 142) showing the path of Uranus among the stars.

Neptune is too low in the west in the evening to be well seen.

MERCURY.

Date. 1891.	R. A. h m	Decl. ° ′	Rises. h m	Transits. h m	Sets. h m
Apr. 25.....	3 18.6	+ 21 08	5 32 A. M.	1 04.7 P. M.	8 38 P. M.
May 5.....	3 15.5	+ 19 09	4 59 "	12 22.1 "	7 46 "
15.....	2 56.2	+ 14 55	4 19 "	11 23.6 A. M.	6 28 "

VENUS.

Apr. 25.....	23 54.6	− 2 11	3 46 A. M.	9 41.1 A. M.	3 36 P. M.
May 5.....	0 38.5	+ 2 17	3 33 "	9 45.6 "	3 58 "
15.....	1 22.8	+ 6 46	3 20 "	9 50.5 "	4 21 "

MARS.

Apr. 25.....	4 07.0	+ 21 36	6 18 A. M.	1 52.8 P. M.	9 28 P. M.
May 5.....	4 35.7	+ 22 48	6 01 "	1 42.3 "	9 24 "
15.....	5 05.0	+ 23 40	5 46 "	1 32.0 "	9 18 "

JUPITER.

Apr. 25.....	22 48.5	− 8 35	3 07 A. M.	8 35.9 A. M.	2 05 P. M.
May 5.....	22 55.2	− 7 57	2 31 "	8 03.3 "	1 35 "
15.....	23 01.2	− 7 22	1 50 "	7 29.9 "	1 04 "

SATURN.

Apr. 25.....	10 51.5	+ 9 35	1 54 P. M.	8 36.2 P. M.	3 18 A. M.
May 5.....	10 50.7	+ 9 38	1 14 "	7 56.1 "	2 38 "
15.....	10 50.6	+ 9 37	0 35 "	7 16.6 "	1 58 "

URANUS.

Apr. 25	13 49.0	− 10 38	6 12 P. M.	11 33.2 P. M.	4 54 A. M.
May 5.....	13 47.5	− 10 29	5 31 "	10 52.4 "	4 14 "
15.....	13 46.0	− 10 21	4 49 "	10 11.6 "	3 34 "

NEPTUNE.

Apr. 25.....	4 14.8	+ 19 40	6 35 A. M.	2 00.7 P. M.	9 27 P. M.
May 5.....	4 16.2	+ 19 44	5 56 "	1 22.8 "	8 49 "
15.....	4 17.5	+ 19 48	5 18 "	0 45.0 "	8 12 "

THE SUN.

Apr. 25.....	2 11.7	+ 13 16	5 00 A. M.	11 57.9 A. M.	6 56 P. M.
May 5.....	2 49.8	+ 16 19	4 45 "	11 56.6 "	7 08 "
15.....	3 28.9	+ 18 56	4 33 "	11 56.2 "	7 20 "

THE MOON.

Apr. 20.....	11 55.7	+ 5 44	3 20 P. M.	10 00.3 P. M.	4 29 A. M.
25.....	15 53.2	− 19 48	8 46 "	1 37.6 A. M.	6 22 "
30.....	19 53.7	− 24 58	12 48 A. M.	5 21.7 "	9 46 "
May 5.....	0 43.2	− 0 04	3 44 "	9 50.7 "	4 10 P. M.
10.....	5 28.9	+ 24 51	6 12 "	2 16.1 P. M.	10 26 "
15.....	10 06.1	+ 17 16	11 01 "	6 32.8 "	1 52 A. M.

Occultations Visible at Washington.

Date.	Star's Name.	Magnitude.	IMMERSION. Wash. Mean T. h m	IMMERSION. Angle f'm N. P't. °	EMERSION. Wash. Mean T. h m	EMERSION. Angle f'm N.P't. °	Duration. h m.
Apr. 18...	*i* Leonis	5.7	11 08	134	12 22.7	302	1 15
22...	65 Virginis	6.1	6 12	104	7 15.4	319	1 04
22...	66 Virginis	6.0	7 00	115	8 11.5	313	1 11
25...	λ Libræ*	5.1	10 08	107	11 20.1	313	1 12
May 2...	r^1 Aquarii†	5.8	14 42	20	15 21.4	301	0 39
12...	*x* Geminorum†	3.6	10 39	130	11 25.7	256	0 46
14...	B.A.C. 3206	6.3	8 35	142	9 42.6	273	1 08

* Immersion below the horizon of Washington.
† Multiple star.

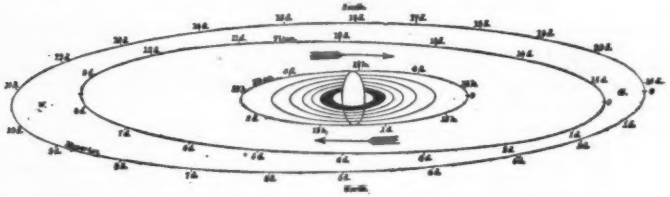
Mr. Marth's Ephemerides of Saturn's Satellites.

[Reduced to Central Time: from Monthly Notices, Vol. LI Nov. 1890; Di = Dione; En. = Enceladus; Mi. = Mimas; Rh. = Rhea; Te. = Tethys; Ti. = Titan; c = conjunction with the center of planet; f = conjunction with following end of ring; p = conjunction with preceding end: n = north; s = south of the major axis of the ring; e = eastern elongation: w = western elongation.]

April	15	12.1 a. m.	En. pn	April	26	3.7 p. m.	Mi. fn	May	7	7.9 p. m.	Te. ps
		1.9 p. m.	Di pn			7.1	Te. fs			9.3	Rh. fn
		2.2	Mi. pn			9.6	Mi. pn			11.1	Mi. fn
		4.3	Rh. pn			11.3	Rh. fs			11.9	Di. w
		5.5	En. fs		27	1.6	Di. ps		8	4.3	Mi. fs
		7.1	Te. e			2.3	Mi. fn			5.2	En. ps
		7.6	Mi. ps			5.8	Te. pn			6.6	Te. fn
	16	2.9	Rh. w			6.1	En. ps			8.2	Di. fs
		2.9	Di. ps			8.2	Mi. pn			9.7	Mi. fn
		5.8	Te. w			9.4	Di. fs			11.6	En. fs
		6.2	Mi. ps			9.9	Rh. e		9	3.0	Mi. fs
		7.1	En. ps		28	4.4	Te. fs			4.0	En. pn
		10.7	Di. fs			4.9	En. pn			5.3	Te. ps
	17	1.5	Rh. ps			6.8	Mi. pn			8.4	Mi. fn
		4.4	Te. e			8.5	Rh. fn			9.2	Di. fn
		4 8	Mi. ps			10.5	Di. fn		10	12.3 a. m.	Te. fs
		5.9	En. pn		29	3.1	Te. pn			3.3 p. m.	Te. fn
		10.5	Rh. fs			5.5	Mi. pn			5.5	Di. w
		10.7	Mi. fs			6.8	Di. w			6.6	En. fn
		11.8	Di. fn			7.5	En. fn			7.0	Mi. fn
	18	12.2 a. m.	Te. fn			10.9	Mi. ps			10.9	Te. pn
		3.1 p. m.	Te. w			10.9	Te. w		11	1.9	Di. fs
		8.1 " "	Di. w		30	1.7	Te. fs			2.6	Te. ps
		9.1	Rh. e			3.1	Di. fs			5.4	En. fs
		10.9	Te. ps			4.1	Mi. pn			5.6	Mi. fn
	19	11.4 a. m.	Tit. sup c. s. 18″			6.3	En. fs			9.6	Te. fs
						7.5	Mi. ps			11.5	Mi. pn
		1.7p. m.	Te. e			7.5	Te. e		12	2.4 a. m.	Di. e
		4.4	Di. fs	May	1	4.2	Di. fn			1.2 p. m.	Te. fn
		7.7	Rh. fn			8.1	Mi. ps			2.9	Di. fn
		9.5	Te. fn			8.2	Te. w			4.2	Mi. fn
	20	12.4	Te. w			8.8	En. ps			6.7	Rh. pn
		5.5	Di. fn		2	6.7	Mi. ps			7.9	En. ps
		9.2	Te. ps			6.8	Te. e			8.2	Te. pn
	21	1.8	Di. w			7.6	En. pn			10.1	Mi. pn
		2.2	En. fn		3	5.3	Mi. ps			10.7	Di. pn
		5.2	Mi. fs			5 5	Te. w		13	2.3 a. m.	En. fs
		6.8	Te. fn			5.9	Rh. pn			2.4	Tit. inf c. n. 18″
		8.5	En. fn			9.3	Di. e				
	22	10.6	Mi. fn			10.1	En. fn			2.8 p. m.	Mi. fn
		1.0	En. fs			11.2	Mi. fs			5.3	Rh. w
		3.8	Mi. fs		4	2.6	En. ps			6.7	En. pn
		5.5	Te. ps			3.9	Mi. ps			6.9	Te. fs
		9.2	Mi. fn			4.2	Te. e			8.7	Mi. pn
		10.6	Di. e			4.5	Rh. w			11.8	Di. ps
		11.0	En. fn.			5.7	Di. pn		14	2.1 a. m.	Mi. ps
	23	12.5 a. m.	Te. fs			8.9	En. fs			2.7	Te. e
		4.1 p. m.	Te. fn			9.9	Mi. fs			3.9 p. m.	Rh. ps
		6.9	Di. pn		5	9.7 a. m.	Tit. sup c. s. 18″			5.5	Te. pn
		11.1	Te. pn							7.4	Mi. pn
	24	2.8	Te. ps			2.8 p. m.	Te. w			8.1	Di. e
		5.1	Rh. pn			3.1	Rh. po			9.2	En. fn
		8.0	Di. ps			6.7	Di. ps		15	12.8 a. m.	Mi. ps
		9.8	Te. fs			8.5	Mi. fs			12.9	Rh. fs
	25	1.4	Te. fn			10.6	Te. ps			1 4	Te. w.
		3.7	Rh. w		6	1.5	Te. e			1.7 p. m.	En. ps
		4.3	Di. e			3.0	Di. e			4.2	Te. fs
		4.8	En. fn			3.9	En. fn			4.4	Di. pn
		5.1	Mi. fn			7.1	Mi. fs			6.0	Mi. pn
		8.4	Te. pn			9.3	Te. fn			8.6	En. fs
		11.0	Mi. pn			10.3	En. pn			11.4	Mi. ps
		11.2	En. pn			10.7	Rh. e			11.5	Rh. e
	26	2.3	Rh. ps		7	2.7	En. fs			12.0 midn.	Te. e
		3.6	En. fs			5.7	Mi. fs				

Two new minor planets of the thirteenth magnitude have been discovered since our last issue, No. 307 by Millosevich at Rome, March 1, and No. 308 by Charlois at Nice, March 5. No. 307 was in R. A. $10^h\ 47^m\ 29^s$, Decl. $+11°\ 37'$; and No. 308 in R. A. $10^h\ 1^m\ 26^s$; Decl. $+19°\ 42'$. Both were moving north and west.

XUM



APPARENT ORBITS OF THE SEVEN INNER SATELLITES OF SATURN, MAY 17, 1891, AS SEEN IN AN INVERTING TELESCOPE.

(*The Vertical Scale is Twice the Horizontal One.*)

Phases and Aspects of the Moon.

			Central Time. d	h	m	
First Quarter	1891	Apr.	15	7	40	P. M.
Apogee	"	"	19	5	24	A. M.
Full Moon	"	"	24	12	05	"
Last Quarter	"	"	1	7	51	"
Perigee	"	"	5	2	36	"
New Moon	"	"	8	12	15	"
First Quarter	"	"	15	1	04	P. M.

Minima of Variable Stars of the Algol Type.

[The times are given, to the nearest hour of Central Time, of only those minima which can be observed in the United States.]

U CEPHEI.

R. A.....$0^h\ 52^m\ 32^s$
Decl.....$+81°\ 17'$
Period.....$2^d\ 11^h\ 50^m$

Apr. 18	8 P. M.
23	7 "
28	7 "
May 3	7 "
8	6 "
13	6 "

R CANIS MAJ.

R. A.....$7^h\ 14^m\ 30^s$
Decl.....$-16°\ 11'$
Period.....$1^d\ 3^h\ 16^m$

Apr. 21	8 P. M.
22	11 "
29	7 "
30	10 "
May 8	9 "

S ANTLIÆ.

R. A.....$9^h\ 27^m\ 30^s$
Decl.....$-28°\ 09'$
Period.....$7^h\ 47^m$

Apr. 21	10 P. M.
22	10 "
23	9 "
24	8 "
25	8 "
May 3	10 "
4	9 "
5	9 "
6	8 "
15	10 "

δ LIBRÆ.

R.A.....$14^h\ 55^m\ 06^s$
Decl.....$-8°\ 05'$
Period.....$2^d\ 07^h\ 51^m$

Apr. 18	10 P. M.
25	9 "
May 2	9 "
9	9 "

U CORONÆ.

R. A.....$15^h\ 13^m\ 43^s$
Decl.....$+32°\ 03'$
Period.....$3^d\ 10^h\ 51^m$

April 16	4 A. M.
23	1 "
29	11 P. M.
May 6	9 "

U OPHIUCHI.

R. A.....$17^h\ 10^m\ 56^s$
Decl.....$+1°\ 20^m$
Period.....$0^d\ 20^h\ 08^m$

Apr. 16	5 A. M.
17	1 "
17	9 P. M.
22	1 A. M.
22	9 P. M.
27	2 A. M.
27	8 P. M.
May 2	3 A. M.
2	11 P. M.
7	4 A. M.
7	12 midn.
12	5 A. M.
13	1 "
14	9 P. M.

COMET NOTES.

Ephemeris of Comet 1890 II (Brooks, March 19). From Bidschoff's elements as given in A. N. Vol. 124, p. 301, I have computed the following ephemeris:

Gr. M. T.	App. R. A.	App. Dec.	Log r	Log Δ
	h m s	° ′		
April 1.5	9 50 38	+34 4	0.5965	0.5193
3.5	47 59	33 48		
5.5	45 30	33 32	0.6006	0.5312
7.5	43 11	33 16		
9.5	41 0	33 1	0.6042	0.5427
11.5	38 57	32 44		
13.5	37 2	32 28	0.6080	0.5545
15.5	35 16	32 11		
17.5	33 38	31 54	0.6117	0.5663
19.5	32 7	31 38		
21.5	30 44	31 21	0.6154	0.5779
23.5	29 28	31 4		
25.5	28 18	30 47	0.6191	0.5894
27.5	27 14	30 30		
29.5	26 16	30 14	0.6228	0.6008

The theoretical light at the end of April is 0.12, that at discovery being unity.

O. C. WENDELL.

Harvard College Observatory, March 14, 1891.

Ephemeris of Wolf's Periodic Comet 1884 III. We take from the *Astronomical Journal* the following ephemeris for the return of this periodic comet, computed by Dr. Berberich and communicated to the *Journal* by Mr. E. E. Barnard:

EPHEMERIS FOR BERLIN MIDNIGHT.

1891	App. R. A.	App. Decl.	log Δ	Brightness.
	h m s	° ′		
May 3	22 31 40	+13 03.9	0.3593	1.32
7	40 19	14 00.6		
11	49 04	14 57.4	0.3401	1.51
15	22 57 54	15 54.2		
19	23 06 51	16 50.9	0.3204	1.73
23	15 59	17 47.3		
27	25 04	18 43.1	0.3001	1.98
31	34 21	19 38.1		
June 4	43 45	20 32.0	0.2793	2.27
8	23 53 17	21 24.7		
12	0 02 57	22 15.8	0.2580	2.62
16	12 45	23 05.0		
20	22 40	23 52.0	0.2362	3.00
24	32 44	24 36.6		
28	42 56	25 18.4	0.2139	3.45
July 2	0 53 15	25 57.3		
6	1 03 42	+26 32.2	0.1911	3.96

Variation corresponding to — 1d in the period

May 11 + 96ˢ, + 1′.4.

June 20 + 133ˢ, — 2′.3.

NEWS AND NOTES.

We suggest to subscribers that it is not the safest way to remit dues to THE MESSENGER, by currency or postal notes, though the latter is a very common way, and the former frequent. If letters are lost or destroyed in railway accidents the remittances are liable to be a total loss to somebody. If post office orders, bank drafts or checks are used the probability of loss is considerably diminished.

Publication No. 2 of Carleton College Observatory is nearly ready for distribution. This number consists of the observations and the reduction of the same for the determination of the three following longitudes:—

I. The longitude of Carleton College Observatory, re-determined, as based on Washington directly.

II. The longitude of Iowa College Observatory at Grinnell, Iowa, also based on Carleton College Observatory.

III. The determination of the longitude of the point where the 103° meridian west of Greenwich intersects the Northern Pacific Railroad, as an initial point for the government survey of the Sioux Reservation. The basis of this determination was Carleton College Observatory.

"*The Irrepressible Conflict*" is the title of the leading article of this issue. The manner in which Professor Boss has presented the general theme in this paper will quicken the interest of the friends of astronomy in the United States, and we hope sufficiently arouse the attention of the public mind generally to a matter that needs radical reform.

The Origin of Stellar Systems. Mr. See, of the University of Berlin, has favored THE MESSENGER by two important papers on the "Origin of the Stellar systems," which, we think, will be read generally with much interest. If we had had, in this issue, space at command, we would have presented some valuable tables from which Mr. See derives some of his data for support of the new theory. We notice, however, that the tables referred to have already appeared in connection with a similar paper published by the author in *Observatory*, for February of this year.

We are sorry also that our crowded pages have, for this month, shut out a large amount of useful amateur matter that we had planned to present. We are now ready to take up the work of which we have previously spoken as adapted to the opera-glass and the small telescope for our numerous friends who desire to do something in this way systematically. The more we have thought of this useful branch of work, the more it has seemed to us that we ought to publish this matter separately and in suitable and convenient form for the use of those interested. Further consideration may lead us to do it and to undertake the work in that form quite soon.

XUM

Polar Snow Caps and a Solar Belt on the Moon. A suggestive article is found in the *English Mechanic* of March 13, 1891, on the probability of polar snow caps and a solar belt on the moon, written by S. E. Pearl, of India. It is assumed that the axis of the moon has changed about 30° in early ages of lunar development, occasioning thus a new tidal equator, By drawing the north pole towards us about 30°, it is noticeable that Maria Imbrium, Serenitatis, Crisium and Smythii lie truly in a line of a great circle and form a chain, in such a manner and at such a place as seem to preclude the possibility of accident. In regard to this fact Mr. Pearl says: "This great chain of *Maria*, therefore I take to be the remains of the great solar belt about the former old (tidal) equator; the remains of the belt of solar influence, the least areas left liquid ere the polar caps united. The enormous littoral ranges are the snow ramparts raised during the long, final struggle around these vast lagoons, repeating exactly on a large scale the features we see in all the other circular formations called craters and walled plains. In case any of your readers may doubt the possibility of the change in the lunar axis of 30°, I may say that when starting this part of the investigation, I laid the case before Professor C. H. Darwin, and ve hery kindly, at once, replied that 'the shifting of the axis of rotation is undoubtedly a mechanical possibility. A large shift, such as you postulate, is far more likely to occur in the case of a body with slow rotation, as in the case of the moon, than in the earth.' "

No. 14, Vol. III, Publications of the A. S. P. is at hand. It contains 83 pages of useful current matter of unusually wide range. It is one of the strongest numbers that the society has issued.

A Remarkable Meteor. By kindness of C. C. Hutchins, of Brunswick, Maine, we have a brief account of a remarkable meteor which was seen near Kingsfield, Maine, on the morning of the 23d of last month. The peculiarities of this meteor were its great size and its vivid light. Its course is said to have been from southwest to northeast, as far as Franklin County, Maine., where it exploded. The noise of the explosion was heard for many miles and shook buildings like an earthquake. It left only a dense cloud of smoke behind, no fragments reaching the earth, so far as known.

Rev. P. B Fisk, of Morrisville, Vt., by private letter, also sends an account of the Maine meteor. He says the explosion took place over eastern Maine, from three to five minutes past 4 o'clock A. M. February 23. Before 4 o'clock, Mr. Fisk was awake, and the full moon shone brightly, and he saw a light like that of a meteor which indicated that it had passed southward and to the east. He was probably 100 miles west of the place where the meteor exploded in Maine.

Dr. Hosmer A. Johnson, of Chicago, died February 26, 1891. He was President of the Chicago Astronomical Society from 1882 to 1889. At the time of his death he was Vice-President, and Professor E. Colbert, of the Chicago *Tribune*, President.

Nebulous System of Orion. By private letter of March 10, C. C. Hutchins says that a portion of the vast nebulous system of Orion is easily visible in a small telescope. Just over and in the same field with ζ Orionis his 6½-inch comet seeker shows a very extensive nebulosity with a mottled light like that of the outlying portion of the great nebula. This is visible out at least half a degree square and is seen to have a great dark rift dividing it into two unequal portions. This same nebulous patch has been seen at Northfield since Mr. Hutchins called our attention to it. The broad, dark rift, in its general course, is concave to the star, and seems to have short, faint branches on either side. The mottling of the nebulous mass is distinctly seen. We used the 8¼-inch Clark equatorial with a power of 50. On December 26, 1889, Dr. H. C. Wilson photographed this region with an ordinary camera attached to a telescope. The lens of the camera was 2½ inches, and the time of exposure was two hours and forty minutes. Two negatives of the same field were taken at different times but the date of the second one was not preserved. Both show the dark rift and portions of the nebulosity above referred to. They were not, however, before observed, simply because the plates were not studied. Some of our books of reference are not now at hand, and hence we can not be sure that this large nebula is not on record. If it is, we do not remember it and we are sure we have never seen it before. This an excellent field for the small telescope and the amateur to try his powers of observation.

Wolshingham Observatory. A star 8.4, red, III type, not in D. M. was observed here January 31st, February 1st and 2d, at $5^h\ 32^m\ 37^s$, $+\ 31°\ 57'$ ('55). This star is perhaps variable.

Circular No. 29.

The variability of Es—Birn. 146 = D. M. + 68.398 has been ascertained. Period and limit unknown. T. E. ESPIN.

Circular No. 30.

Rousden Observatory, England. C. E. Peck, of Rousden Observatory, England, favors THE MESSENGER with a report of astronomical work done during the year 1890. It has consisted chiefly of transit observations and observations of variable stars, a list of which is given. This is the fifth year that variable star work has been carried on regularly.

A Remarkable Solar Halo.—Yesterday at 1 P. M. I witnessed an exhibition of the above phenomenon quite unlike any I had ever seen or read of. The sky had been unusually clear, and I was congratulating myself on the anticipated cloudless night, but at the above hour I was surprised to note the sun shining but dimly, and, looking out of a west window, to see the sky nearly covered with a thin haze, and, in the northwest, at an elevation of about 45°, a segment of a luminous curve. From the lawn, the segment was found to be part of a complete circle, but the sun instead of occupying its center, as is generally the case, was at its southern circumference, the northern limit being, by estimation, contiguous to the pole star. The diameter of the circle must, therefore, have been 90°. The luminous band was about half a degree in width and without color. LEWIS SWIFT.

Warner Observatory, Rochester, N. Y., March 18, 1891.

Star Studies by the Opera-Glass. We were very sorry not to be able this month to begin the star-studies by the opera-glass as we had expected to do. Mr. G. P. Serviss, of New York, will have some matter ready for the next MESSENGER. He has been unavoidably delayed in its preparation so far.

Charles A. Bacon, director of Smith Observatory, Beloit, Wis., is industriously at work on an illuminated globe for the aid of teachers and students in the study of the constellations. Something of the plan of the globe and the method of using it will be given by Mr. Bacon in the next issue of this periodical.

Publications of the Leander McCormick Observatory of the University of Virginia. We are in receipt of Part 5, Vol. 1, of the above named publications, with title Durchmusterung, — 23°. This is a neatly printed catalogue of 6671 stars, all belonging to *one* degree of south declination numbered 23, and is a continuation of the Bonn Durchmusterung. The work was begun at the Cincinnati Observatory, and the instrument employed was a 4-inch equatorial by Alvan Clark & Sons. The instrument used at the Leander McCormick Observatory, where the work was completed, was made by Kahler and was of the same size. The observers were Professor Stone, F. P. Leavenworth, H. C. Wilson, H. V. Egbert and John Jones.

Comets Captured by Electricity. Several copies of the *Examiner* (San Francisco, Cal.), bearing date March 8, have been received from friends in different parts of the West, calling attention to a new mode of discovery of comets by the aid of electricity, and other ingenious appliances, expected to work automatically and constantly. In a word the machine is of such a kind (according to the *Examiner's* report) that if it be set its telescope will sweep the sky automatically and automatically record the observation of cometary bodies and those only. All the observer has to do is to start his machine and it will do the work alone, and he can go about other business or sleep if he chooses. We have the impression that this sensational story, with its strong head-lines, is a hoax, or a tremendous scientific blunder. It is easy to believe this, because the *Examiner* has a wonderful capacity for blundering in scientific matters. We remember our own experience with that paper two years ago. Its manager had asked the favor of some facts about the eclipse. The facts were given, and they elicited an editorial that was the most wonderful piece of blundering known to us in the whole range of newspaper possibilities. The errors were so obvious that no one paid any attention to them. This, probably is another illustration of the *Examiner's* progressive science.

Theology by Starlight is an apt and forceful interpretation of the Word now spoken from on high, by Newton M. Mann, who has written much and well for these pages for years past. The author believes that

> "One sun by day, by night ten thousand shine
> To light us up into the Deity."

XUM

Photographic Notes. "M. Prosper Henry calls attention to the fact that the co-efficient of refraction is not the same for photographic rays as for visual. . . . At Z. D. 80° visual and photographic refractions would differ by about 5″; and generally in photographic work, all visual refractions would be increased in the ratio of 1.0156 : 1. The measures from which this result was obtained were made by placing a grating in front of the object glass of a telescope, and thus obtaining short spectra of stars." —*The Observatory.*

Journal de l'Industrie Photographique suggests a method for developing photographic plates in full light. Developing, washing and fixing are carried on in boxes having red glass sides; plates are transferred from box to box by means of taps.

Mr. Gabriel Lippmann has brought before the Paris Academy of Sciences photographs of the spectrum in which all the colors appear with their exact tones. The photographs are permanent, exposure to light and air producing no apparent effect upon them. The following statements are made by M. Lippmann in the *Photographic Times* of March: "The sensitive coat (on the photographic plate) must be continuous; that is to say, that the sensitive matter must be distributed in a state of division as it were infinite, in a transparent support such as gelatine, albumen, or collodion. In general the bromides of commerce form much too heavy an emulsion for employment. The sensitive coat must be placed against a reflecting surface. For this purpose it is fixed during the exposure in a hollow frame containing mercury, which forms a plane mirror in contact with the sensitive coating. The exposure is made, then the development, finally the fixing, followed by washing in the ordinary manner with hyposulphite of soda, or cyanide of potassium. The plate has been exposed dry and the colors appear there when it again becomes dry. The theory of the experiment is very simple—the incident rays forming the image in the camera interfere with the rays reflected by the mercury; there results in the interior of the sensitive coat a series of fringes of interference, that is to say, of maximums of light separated by entirely obscure minimums. Only the maximums imprint the plate; the photographic operations terminated, they are marked by a series of transparent coats of silver reduced, separated by the interval itself which separated two maximums, that is to say, equal to a half length undulation. . . . Seen by transparency the cliché is negative, that is each color is replaced by its complementary color—green by red, red by green, etc. I have already found that the colors of the cliché may remain with impunity exposed either to the light of day or to the concentrated rays of a powerful electric arc."

The English of the above translation is not very choice; but we prefer to let it stand as it is.

At the Lick Observatory Jupiter was photographed on thirty-three nights between July 14 and November 3, 1890.

Mr. Isaac Roberts concludes from photographs taken on ten different nights that the nucleus in the Great Nebula in Andromeda is variable.

Photography of the Solar Prominences. A useful paper on the photography of the solar prominences will be found in *Technology Quarterly*, Vol. 3, No. 4, November 1890, from the pen of George E. Hale, Kenwood Physical Obsrevatory, Chicago, Ill.

Barnard's Photograph of the Milky Way.—In No. 492, Vol. 21 of the *Photographic Times* of New York (Feb. 20) will be found a reproduction of a photograph of a portion of the Milky Way by E. E. Barnard, of Lick Observatory, and a descriptive article accompanying the same. The negative was taken Aug. 1, 1890, with a six-inch portrait lens, and the position of the center of the picture is right ascension $17^h\ 55^m$, and south declination 28°. The exposure was three hours and seven minutes. The important feature that is claimed for the superiority of the Lick photographs of the Milky Way is that they show the structure of it better than any other means of study. It is believed by astronomers that a better knowledge of the Milky Way will give the key to the structure of the Universe, and this is the place to begin severe and exhaustive study. By the use of the telescope with high power the details of a field large enough to give knowledge of the structure are lost. The massing of stars, the cloud-like forms and wonderful nebular structure almost wholly disappear. It is on this account that the portrait lens of larger field has been tried with surprising results when the best quality of sensitive plates have been used in long exposures. It will be of interest to our readers to know that copies of one of these beautiful pictures can be obtained from the *Photographic Times*, 423 Broome St., New York City, at the small price of 25 cents per copy.

Edmund J. Spitta's Recent Papers. We are sorry that we have been unable to find space for some valuable papers by Edmund J. Spitta of England which the author has had the kindness to send us recently. We mention the titles of them that American readers may avail themselves of copies from other sources.

"Some Experiments relating to the method of obtaining the Co-efficient of Absorption of the wedge photometer."

"Some experiments relating to the photometric comparison of points of light with objects of sensible area," and

"On the appearance presented by the satellites of Jupiter during transit, with a photometric estimation of their relative albedoes and of the amount of light reflected from the different portions of an unpolished sphere."

Baron D'Engelhardt's Private Observatory. We have recently received two fine volumes of observations made by Baron D'Engelhardt in his private Observatory at Dresden. The first volume was published in 1886, and contains descriptions of the Observatory and instruments and observations of comets, planets, nebulae and star-clusters made with a twelve-inch equatorial and filar micrometer. It is illustrated by four large engravings of the building and instruments. The second volume was published in 1890, and contains the results of four years' observations, all made by the Baron himself with the twelve-inch Grubb refractor, comprising observations of the satellites of Saturn, comets, asteroids, the Bradley stars, a list of twenty-one double stars selected by O. Struve, a number of comparison stars for Comet 1862 III, and 203 nebulae and star-clusters. All of these are micrometrical measurements of a good degree of accuracy and afford a large amount of very valuable material for the study of the motions of the heavenly bodies.

XUM

Some Thoughts on Subjects Astronomical is the title of a recent pamphlet of 58 pages by John Romains, S. S. C. Scot. of Scotland. It is a résumé of the important facts of astronomy stated in a clear and direct way, and well suited to popular readers.

Annual of the Royal Observatory of Brussels. The Annual for 1891 of the Royal Observatory of Brussels, by F. Folie, Director, has been received.

Annual of the National Observatory of Tacubaya, Mexico, has reached our table. This is volume XI and is for 1891. This work is under the Directorship of Felipe Valle.

BOOK NOTICES.

The Geometry of Position, by Robert H. Graham, author of Graphic and Analytic Statics. London and New York, Messrs. Macmillan & Co., Publishers, 1891, pp. 192.

So far as we know there is no work in English devoted to this interesting science of the geometry of position, as it is called by French and German writers, unless we except Salmon's *Conic Sections* and *Higher Plane Curves*, which present, in certain chapters, a similar set of problems. The analytic method used by Salmon, it is claimed, is ill-suited to make prominent the useful aspects of the subject, or, to illustrate fully the beauty and elegance of projective geometrical methods. Carnot published a work on the geometry of position at the beginning of this century. Since that time this branch has been developed by Poncelet, Standt, Steiner, Cremona and others, and the graphic idea has grown in prominence. On this account the study of geometry of position has been made compulsory in the Federal Polytechnic of Zürich, in Strasburg University and other continental institutions. The tendency of later writers seems to be to make the study of the geometry of position an introduction to lessons on graphic studies. The author's experience in reading Culman's *Graphic Statics* is in the same direction, and hence he was led to prepare the work before us, for the advantage of students who wish preparation in such studies.

An idea of the plan of the work may be gathered from the subjects discussed. They are six in number, with titles as follows: Anharmonic Pencils and Ratios, Projective Conics, Reciprocal Figures, Centers of Gravity, Ellipses of Inertia and Kerns and the Elastic Line. Each theme is followed by several pages of examples illustrative of the line of discussion followed. There is much other useful history to be found in the books referred to and in the full preface of this work, showing how the geometry of position has come to occupy the prominent place in some lines of study, that it has recently acquired. So far as appears from a cursory examination of this book, it fills a much needed place in the relations already indicated, and probably will interest American students of mathematics who have given, or wish to give attention to such themes.

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THE NEW ORLEANS MAFIA.

Interest in the New Orleans excitement is as brisk as it was when Chief Hennessey was murdered by the Mafia, and the Camorrists were summarily lynched by the maddened Louisianians. There is no doubt that the members of the Mafia, whose motto, doctrine and watchword is "*Vendetta!*" contemplate vengeance, and it does not require a very vivid stretch of the imagination to assume that some of those who were concerned in the lynching will die by the thrust of a Tuscan stiletto. However, in the peaceful Northwest, where the Mafia wreaks no vengeance, it is of more interest to know that the Saint Paul & Duluth Railroad, with its admirable road and equipment, is the short line between the two sets of Twin Cities, St. Paul, Minneapolis, Duluth and West Superior. Close connection always. Information cheerfully furnished by Geo. W. Bull, General Passenger Agent, or Geo. C. Gilfillan, Asst. G. P. A., St. Paul, Minn.

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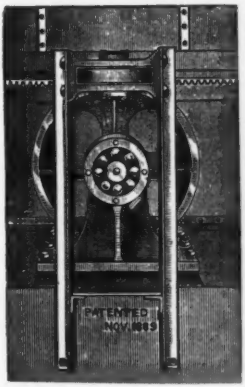

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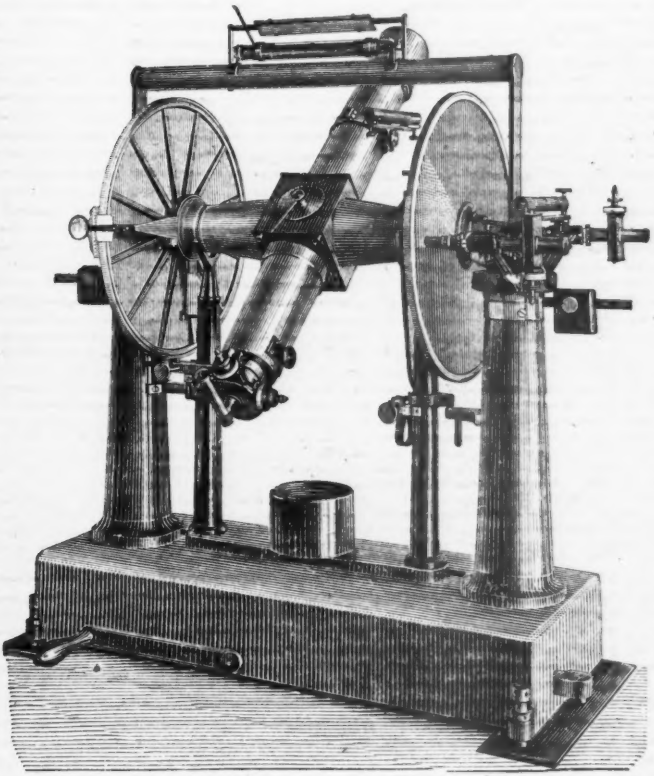

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